

OBITUARY NOTICE

STANLEY J. F. PHILPOTT

1888-1952

In Dr Philpott the British Psychological Society found one of its most devoted and ablest servants during its formative period. From 1929 until his death he gave himself wholeheartedly to its service in a variety of offices. For the greater part of this time he was Treasurer, and did more than any other to establish the finances of the Society. He was an extremely good man of business, one who could keep details constantly in mind, and one who was very firm on routine without lacking the constructive imagination to deal with new problems. Only those who had the privilege of then working with him can understand how much the Society owed to him during the lengthy preparations for incorporation. We faced the complicated task of drawing up a constitution which would satisfy the Society's members and the Board of Trade, and he brought to it a finely logical mind and considerable powers of negotiation and argument. It was he who in the end succeeded in convincing the Board of Trade that we and no others had a right to the title we had already used for a quarter of a century. In 1948 he was elected President. He served psychology in other spheres. He served the full range of offices in Section J of the British Association, and was a member of the Council of the Association at the time of his death. Further, he was associated with *The British Journal of Educational Psychology* from the beginning.

Philpott was trained as a teacher at Exeter, and he retained some touch with schools until the end. He lectured to various training colleges, and took part in pupil selection, employing here Intelligence Tests of his own construction and standardization. He was destined to teach. 'Once a teacher, always a teacher', he said, and so not even the prospect of more time for his researches could lure him from teaching. He won the devotion and affection of his students, for, as one of them (Dr F. W. Warburton) says, he thought of them first and foremost as people. He was able to induce them to undertake with real enthusiasm some of the most tedious experimentation that ever psychologist engaged in. By their aid he developed his original work on the rhythmical fluctuations of mental work. There was an enormous volume of work carried out under his direction: its scientific value cannot be discussed in this article.

He devoted the most vital years of his life to the study of 'waves'. His monograph on 'Fluctuations in Human Output' (*British Journal of Psychology: Monograph Supplement*, no. 17, 1932) laid the foundations for what followed. By laborious experimentation and no less laborious theoretical analysis he claimed to have established a fundamental psychological law, that mental efficiency fluctuates in a complex system of waves whose lengths have to be stated in logarithmic time units. Looking back over the years that followed its publication, it must be said that the psychological world has avoided rather than challenged his conclusion. He, with the patient determination that characterized him, tried over and over again to drive his point home, but it was too novel and too abstract to be either criticized in an informed manner, or accepted. It may be that Philpott, brooding over his concepts for so many years, came to see them as simpler than they were, and so failed to make concessions to other minds: perhaps he prematurely

made known some of the consequences he drew from the basic theory, and thereby started hares which diverted his readers from the real trail. Certainly even those kindly disposed to him and his ideas found it all very difficult (though it is reasonably simple to those who are content to take it as simple) and were tempted to give it up. Others just ignored it.

It is beyond question that Philpott was deeply hurt by this attitude, though never embittered. He continued persistently to seek for criticism (and he was never one to regard himself as inevitably in the right), and when he failed to obtain it, he would put in a few days of strenuous gardening, and then return to the attack. To his very great joy, and the joy came all but too late, the *British Journal of Psychology* devoted in 1952 nearly a full issue to a discussion of his views. Dr Lewis Richardson, an old friend whom he greatly admired, rose to the attack and, in his happiest hours, Philpott replied vigorously. But even here it would seem that Richardson's criticisms centred more on the secondary, and possibly vulnerable, deductions from the theory than upon the foundations. So his life-work remains unassimilated, awaiting perhaps destructive criticism of the experimental technique or of the statistical analysis: it cannot, surely, be always left in timid neglect. Philpott came to psychology from the physical sciences, and he had little sympathy with those who excuse the shortcomings of psychology by the complexity of its subject-matter. He knew that physicists were attacking greater and more abstract complexity with greater boldness, and he was not afraid to commit himself in like manner.

Philpott was a most lovable man. Life had never been easy for him, but he never lost courage, nor cheerfulness, however great the burden of work he had to carry. He judged others shrewdly, but seldom failed to find good qualities in them. He was a friend to be valued. I have spoken of him as psychologist and as one who worked so well for the Society, but to his friends he was above all else just a fine man, a simple-hearted gentleman. The outstanding memory which those friends retain of him is his complete integrity, in every sense which that word can bear.

A. W. WOLTERS

THE CONCEPT OF HEED

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I. INTRODUCTION

Do the words and expressions which the subject uses when he makes his introspective report, refer to internal events going on inside him? If they do, it is difficult to see why we should not use the subject's statements in order to formulate and verify hypotheses about such processes. If they do not it is difficult to see what reason we should have for believing in the existence of the sort of events which are described in the text-books of introspective psychology. If, as is suggested in this paper, some of them do and some do not, it becomes extremely important for the psychologist to be able to discriminate between the two cases.

Now in so far as the language of the introspective report is the 'psychological' language of ordinary speech, this is the question which has recently been exercising the minds of the philosophers; and in at least one case (Ryle, 1949) the conclusion that has been reached is preponderantly negative. In his book *The Concept of Mind* Ryle has attempted to show that the traditional view which holds that mental states and processes are private internal occurrences within the individual is mistaken. He does not deny that some of the statements which we ordinarily make about people, refer to states and activities of the individual that are 'private' or 'covert' in the sense that only the individual himself can report their occurrence. He would maintain, however, that such statements constitute only a small minority of the statements we make about our own and other people's minds.

I shall argue in this paper that the number of mental concepts which do entail a reference to covert states and activities of the individual is much larger than Ryle is prepared to admit. In particular it will be contended that a reference of this kind is involved in our ordinary use of such expressions as 'being conscious of' or 'paying attention to something', 'observing', 'watching', 'looking', 'listening', 'seeing', 'hearing', 'smelling', 'tasting', 'feeling', 'noticing', 'perceiving' and 'recognizing'.

II. THE CONCEPT OF HEED

The key notion in Ryle's account of what is traditionally referred to as our 'apprehension of the external world' is the concept of 'heed'. As he himself points out (p. 136), this notion of 'heed', or 'heeding' is closely related to the concept of 'consciousness' which is the basic concept in all the traditional theories of mind. While heed does not carry quite the same theoretical load as does the notion of consciousness in the traditional theories, it is employed by Ryle in his analysis of a wide range of mental concepts, and a large part

of his case against the view that mental concepts entail a reference to covert states, processes and activities, would seem to depend on his ability to show that paying heed is not a covert activity.

Ryle defines the notion of 'heeding' or 'minding' as embracing such concepts as 'noticing, taking care, attending, applying one's mind, concentrating, putting one's heart into something, thinking what one is doing, alertness, intentness, studying and trying'. Concepts which 'entail, but are not entailed by, heeding' include 'enjoying, disliking, pondering, searching, testing, debating, planning, listening, relishing, calculating and scrutinizing' (p. 136), looking (p. 232), observing, watching, desiring (p. 207) and recognizing (p. 223). Remembering something, according to Ryle (pp. 91 and 137-9) involves having paid heed to it at the time, while being conscious of sensations in one's body or objects in one's environment is evidently synonymous with heeding or noticing them (pp. 157-8). It will be seen from this that Ryle's concept of heed corresponds more closely to the traditional concept of attention than to that of consciousness. On the traditional theories consciousness is the basic notion of which attention is a special active or conative form. For Ryle on the other hand, it is 'attending' or 'heeding' which is the basic concept, and no distinction is drawn between paying attention to something and being conscious of it.

III. THE CONTEMPLATIVE THEORY OF HEED

The traditional or, as Ryle calls it, the 'contemplative' theory of heed or attention and consciousness in the form in which I wish to defend it, may be stated as follows. The expression 'paying attention' refers to an internal activity of the individual presumably of a non-muscular variety whereby he exercises a measure of control over the vividness or acuteness of his consciousness of (a) the sensations to which he is susceptible at that moment, or (b) such features of the environment as are impinging on his receptors, without necessarily adjusting his receptor organs or their position in any way. In paying attention to something the individual is regulating the vividness of his consciousness of the object or sensation in question and hence the number of its features of which he is conscious. The expression 'being conscious of something' refers to a peculiar internal state of the individual which normally accompanies any reasonably intense stimulation of his receptor organs, the particular form assumed by the individual's state of consciousness at a given moment being determined by the pattern of physical energies impinging on his receptor organs at the time.

Being conscious of something is by definition a necessary condition of the individual's being able to give a first hand report on that something either at the time or later. It is not, however, a sufficient condition of the individual's ability to make such a first hand report, since it is possible for someone to be conscious of things which he cannot put into words, without his actual capacity to verbalize being in any way disturbed. Likewise, though here the relationship is probably contingent rather than necessary, the successful performance of any skilled activity depends to a greater or lesser extent on the individual paying attention to, i.e. maintaining a vivid consciousness of, relevant features of the situation and his own activity with respect to it; but the mere fact that someone is paying attention to what he is doing does not entail that the performance will be adapted to the demands of the task.

IV. RYLE'S OBJECTIONS TO THE CONTEMPLATIVE THEORY

Ryle's first objection to this type of theory (pp. 136-7) is that it leads to a *reductio ad absurdum* in those cases where we speak of watching carefully or attentively. He points out that it is always possible to ask of a spectator, whether he has been a careful or a careless one. In order to interpret this on the contemplative view, he suggests, we should have to postulate an additional process of watching his watching, which is present in the careful spectator and absent in the careless one. This interpretation leads to an infinite regress, since it would always be sensible to ask whether or not this watching of one's watching was done carefully or not. There is, however, no reason why the contemplative view should force us to adopt this particular interpretation. As Ryle himself points out (p. 136), minding can vary in degree. There is, therefore, no reason why we should not say that the difference between the careful and the careless spectator lies in the amount of heed that each pays to the scene before his eyes. The careless spectator is not one who fails to watch his watching, nor is he completely oblivious of what is going on, he merely pays insufficient heed to it. What distinguishes the careful spectator from the careless one are the detailed and accurate reports which he is able to furnish as a result of the richness and vividness of the impressions with which his more active heed-paying provides him.

Ryle's second objection to the traditional theory of attention is that it fails to account satisfactorily for those cases where we speak of applying our minds to some task, such as whistling or driving a car. In this case, he argues (p. 138), we are not doing two things, whistling and minding, driving the car and attending to our driving; we are performing a single activity in a certain way. He points out in support of this contention that we cannot stop driving the car and continue our heed-paying. This argument, however, is singularly unconvincing. The fact that we cannot stop driving and continue our heed-paying merely shows that we cannot continue to pay heed to something that is no longer there to pay heed to. We do not normally lapse into unconsciousness after applying the hand-brake; we turn our attention to other things. On the other hand the fact that one can, if one is sufficiently foolhardy, continue to drive and cease to pay heed to what one is doing would suggest *prima facie* that there are two distinct processes going on here. Ryle is doubtless right in pointing out that in driving with care one is not doing two things at once in quite the same sense as one is when one is walking along and humming at the same time. In humming and walking at the same time one is performing two distinct sets of muscular movements simultaneously. When heeding and driving occur together, on the other hand, there is only one set of muscular movements, those of manipulating the controls; and in that sense there is only one activity being performed. No one, however, supposes that heed-paying is a separate set of muscular movements occurring alongside the muscular movements involved in driving. Nor is heeding thought of as an unrelated activity going on at the same time as the driving. It is a peculiar sort of internal activity which controls the movements of the driver's limbs, by regulating his consciousness of the stimuli to which he responds.

V. THE DISPOSITIONAL THEORY OF MENTAL CONCEPTS

Although Ryle has failed to produce any conclusive objections to the contemplative theory, it is clear that if he can give a plausible account of the logic of 'heed concepts' which dispenses with the assumption that they refer to peculiar private events within

the individual, we should undoubtedly be led to prefer such a theory on the grounds of parsimony. We must therefore examine the account which Ryle has offered of the logic of these concepts in order to discover whether or not it constitutes a satisfactory alternative to the traditional view.

The peculiarity of mental concepts as a class is that in order to determine whether or not someone knows, believes, understands, recognizes, remembers, wants, feels, is enjoying, attending to or thinking about something, you either have to cross-examine him or else observe considerable stretches of his behaviour before you can settle the question with any degree of confidence. This logical peculiarity is traditionally explained on the assumption that these mental concepts refer to invisible states and processes within the individual, whose existence and nature can only be determined with certainty by the individual in whom they occur, although it is usually possible also for an external observer to make reliable inferences about them by observing the behaviour to which they give rise.

Ryle's explanation of this logical feature is quite different. He supposes that mental concepts, or at least most of them, refer to what may be called behavioural dispositions, i.e. capacities, tendencies or temporary dispositions to behave in a certain way. To assert that someone has a capacity or tendency to behave in a certain way on this view is not to say anything about what is going on here and now, it is to assert a hypothetical proposition about how the individual could or would behave if certain circumstances were to arise. Hypothetical propositions of this kind can only be verified by investigating the behaviour of the individual under the conditions supposed. The proposition '*X* can swim', for example, can only be verified by observing *X*'s behaviour when in the water. Similarly with the proposition '*X* knows the date of the battle of Salamis': unless you happen at that moment to hear *X* say '*Salamis* was fought in 480 B.C.', you would either have to wait for the chance of hearing his reactions when called upon to exhibit his knowledge of ancient history, or else adopt the more practical course of testing his knowledge by asking the appropriate question. The reason why it is often necessary to cross-examine the individual in order to discover what he knows, is not that knowing is a peculiar internal state or activity of the individual of which he alone is directly apprised; it is that an important part of what we mean when we say that *X* knows the date of the battle of Salamis is that he can give you the correct date when asked to do so.

With a few notable exceptions of which 'cogitating', 'visualizing' and 'having sensations' are the most important, Ryle attempts to apply this type of explanation to all the mental concepts treated in his book. In most cases moreover the attempt has proved remarkably successful. To my way of thinking there can be little doubt that the dispositional account which he gives of such concepts as 'knowing', 'believing', 'understanding', 'recognizing', 'remembering', 'intending' and 'wanting' is substantially correct. It is only with his attempt to apply it to such concepts as 'attending to', 'observing' and 'being conscious of something' that I wish to quarrel.

VI. RYLE'S APPLICATION OF THE DISPOSITIONAL THEORY TO HEED CONCEPTS

Ryle contends (pp. 137-9) that to say that someone is paying attention to what he is doing entails that he has at least two important dispositions, (*a*) the disposition under favourable circumstances to remember and give a first hand report on what it is he has been paying heed to, and (*b*) the disposition to adapt his performance to the various

demands of the task as they arise. Now it is quite true that if we are told that someone is paying close attention to what he is doing, we normally expect him to be able to answer questions about his activity and to have made at least a better showing at the activity than if he had not been applying his mind to the same extent. It is also true, as Ryle points out, that we frequently conclude from the fact that someone is unable to answer questions about something that has been said in his presence, or from his failure in certain skilled performances, that he has not been paying attention to what was said or to what he was doing. But it does not follow from this that to say that someone is paying attention entails that he has the disposition to do these things. A schoolmaster frequently concludes from the fact that a boy has got the wrong answer to a mathematical problem, that he has set about it in the wrong way. Yet this would not lead us to say that to set about a problem in the right way entails a disposition to get the right answer. We only conclude that the boy must have used the wrong method if we know that his capacities are such that he could not have avoided getting the right answer had he used the correct method. Similarly, we only attribute someone's failure in a skilled activity to lack of attention if we know that his capacities are such that no other explanation of his failure is possible. One would hardly expect someone who had never been near an aeroplane before to be able to meet the demands of the task of piloting one, however closely he attended to what he was doing.

On the view which I am urging, the individual who pays attention is more likely to succeed in so far as he becomes acutely conscious of those features of the situation which are relevant to the successful performance of the task. Close attention to his own activity will be of no avail to the unskilled person because he has not learnt to discriminate between the relevant and irrelevant features. On the other hand an acute consciousness of the details of his own activity in relation to the environment may actually detract from the efficiency of performance in the case of an individual who has learnt to make many of the adjustments involved automatically. Thus we frequently say of someone whose skill is already well developed that his performance suffered because he paid too close attention to what he was doing. It is difficult to see what meaning could possibly be attached to this statement on a dispositional theory of attention.

In claiming that 'attending' entails 'being able to say something about what is going on', Ryle is on stronger ground. It is certainly true that it would be extremely odd to say that someone was paying attention to something, but could tell you absolutely nothing about it. But it is arguable that this is merely because one cannot pay attention to something without at least noticing the thing to which one is paying attention. There is no doubt that to say one has noticed something entails that one has the capacity to mention it and point it out, but to say that one has noticed something and to say that one has paid attention to it, are not, as Ryle appears to think, to say the same thing. 'Noticing' is an achievement concept like 'recognizing', 'perceiving', not an activity concept like 'pondering' or 'attending'. 'Noticing', 'perceiving', or 'recognizing', are the achievements that result from the activities of looking, listening and attending. If one looks, listens or attends one normally notices or recognizes something or other, but one can also attend and fail to notice; one can look and fail to see, listen and fail to hear. When we say that we have failed to notice anything, what we really mean is that we have failed to notice anything remarkable or anything additional to what we have already noticed. You can hardly be said to have paid attention, if there was nothing at all which you could be

said to have noticed as a result of your attending. But it does not follow from the fact that *A* notices more about the situation than *B* that *A* was paying closer attention. The man who pays closer attention usually notices more, but the relationship is contingent rather than necessary.

VII. RYLE'S ACCOUNT OF THE LOGIC OF HEED CONCEPTS

The expression 'paying attention to something' exhibits the distinctive logical characteristics which are normally associated with words and expressions which refer to activities. The fact that it is perfectly good sense to speak of someone being engaged in paying attention to something, while it is nonsensical, for example, to speak of someone being engaged in knowing or understanding something, clearly shows that 'paying attention' or 'heed' is an activity expression in contrast to dispositional verbs like 'expect', 'know', 'like', and 'believe' or achievement verbs like 'understand', 'remember', 'recognize', 'perceive' and 'infer' where such a combination would be nonsensical.

It might be objected that 'attending' differs from those verbs which unquestionably refer to activities in that it is not sensible to use it in conjunction with adverbs like 'quickly' or 'slowly'. We can say 'he slowly began to pay attention to his surroundings', but not 'he paid slow attention to his book for five minutes and then rapid attention to the black-board'. There are, however, a number of expressions which can properly be described as 'activity verbs' of which the same is true. For example we can say 'he slowly took hold of the hammer' but not 'he held the hammer slowly for five minutes'. The analogy between 'attending' and 'holding' seems generally very close.

In support of his contention that 'attending' is not an ordinary activity verb, Ryle draws attention to the curious fact that it is always possible to replace a 'heed verb' by a 'heed adverb'. We can speak, to use his examples, of 'reading attentively', 'driving carefully' and 'conning studiously' just as readily as we can of 'attending to the page in front of one', 'taking care in one's driving', and 'applying one's mind to the task of translation' (p. 138). Ryle contends that the adverbial form is the more accurate way of expressing what is meant when a 'heed concept' is used. To say that someone is doing something heedfully, he maintains is merely to say that he is doing it in a certain way or in a certain frame of mind, i.e. with a disposition to adapt his performance to the various demands of the task as they arise and to answer questions about it.

On this theory the fact that 'paying attention' behaves like an ordinary activity word is explained on the assumption that the phrase 'paying attention to something' is analysable into two parts, (a) a categorical statement that a certain activity is taking place, and (b) a hypothetical or dispositional statement about how the individual in question would behave if certain contingencies were to arise. Ryle calls it for this reason a 'mongrel categorical statement'. The categorical part of the statement from which it derives its logical characteristics, is however, extremely uninformative. It asserts merely that some unspecified activity is being performed. In order to discover the nature of this activity we must find out what it is that the individual in question is paying attention to. As Ryle points out (p. 143) to say that someone is paying attention is an incomplete statement, unless we are told or unless it is obvious from the context of the remark what it is that he is paying attention to. On this view the part of the supposed meaning of the phrase which refers to the performance of an activity is strictly speaking redundant, since it must always be supplemented by a specification of the activity which is being performed.

In his lengthy discussion of 'mongrel categorical expressions' (pp. 140-7) Ryle is at pains to try to explain how it is that the fact of attention or inattention can be used to explain the failure or success of the individual in the activity he is performing. He shows convincingly enough that we can explain the bird's flying south by saying that it is migrating, without implying that migration is an additional process superimposed on the activity of flying south. The statement that the bird is migrating explains the behaviour of the bird by bringing the particular behaviour in question under the general rule that birds of certain species change their habitats at certain times of the year. Unfortunately he does not explain how the analogy is to be applied in the case where we explain the failure of an individual to complete a task satisfactorily or to give an adequate report on what was happening, by saying that he was not paying sufficient attention. It is difficult in this case to see what the general rule involved could be, unless it is the rule that if you don't pay attention you won't be able to carry out the activity you are performing satisfactorily or give an adequate first hand report on what went on. On Ryle's analysis of attention this general proposition reduces to the tautology 'unless you are disposed to give a first hand report on what is going on and to carry out what you are doing satisfactorily, you won't be able to give a first hand report on what is going on or carry out the activity you are performing satisfactorily'.

VIII. THE OBJECTION TO RYLE'S ACCOUNT OF THE LOGIC OF HEED CONCEPTS

Although Ryle fails to produce any conclusive reasons for adopting his theory of 'attending' in preference to the traditional account, it is difficult to produce any decisive arguments against it as long as we restrict the discussion to the special case where we speak of someone paying attention to what he is doing. His case breaks down, however, once we try to apply it to those cases where we are said to pay heed to an object in our environment or to some feeling we have, without being engaged in any other activity with respect to it. Ryle castigates the traditional theorists for 'misdescribing heed in the contemplative idiom' (p. 137), but he himself overlooks in developing his own theory the important cases where paying heed to something is purely a matter of watching, listening, observing or contemplating. Ryle explains the fact that 'attending' exhibits the usual characteristics of an activity verb, rather than those of a dispositional verb, on the assumption that verbs like 'attending' and 'heeding' assert the occurrence of the activities which are being performed attentively. There is no special activity called 'attending', there is only the attentive performance of an activity. The logical consequence of this theory is that the individual's own activities are the only sorts of things to which attention can be paid. If Ryle's theory were correct it should be nonsensical to talk of someone paying attention to anything other than an activity which he himself is performing. In fact, of course, we can speak with perfect propriety of the paying attention to any kind of object, phenomenon or sensation which is visible, audible, tangible, or otherwise perceptible. In such cases there is no activity which is being performed attentively or heedfully. To attend in such cases is merely a matter of contemplating or observing the object or phenomenon in question. We cannot say that when we pay heed to something we are watching it, listening to it, observing or contemplating it heedfully, since as Ryle himself points out (pp. 207 and 223), words such as 'watching', 'listening', 'observing' and 'describing' already entail that heed is being paid. These expressions do not refer to activities like driving a car which can be performed with or without heed, they refer to

special forms of the activity of heed-paying itself. It makes nonsense to say that someone was observing, watching, contemplating or listening to something without paying any attention to it, whereas it makes perfect sense to speak of someone driving without paying any attention to what he is doing.

The inadequacy of Ryle's account appears most clearly when we examine the account which he gives of expressions like 'being conscious of', 'observing', 'watching' and 'listening'. To be conscious of the sensations in a blistered heel according to Ryle (pp. 157-8) is to pay heed to them; but what is the activity which is being performed attentively or heedfully here? It would seem from his long discussion of 'observation' (ch. VII) that for Ryle to say that one is observing something is to say that one is paying heed to the sensations derived from it, and 'watching' and 'listening' by the same token, refer to the paying of heed to visual and auditory sensations respectively. But we cannot say that 'having sensations' is the activity which is being performed heedfully in these cases, since to have a sensation itself entails paying at least some heed to the sensation. We can speak of failing to notice the sensations which one would have had if one had paid attention to them; but to say that one had a tingling sensation in the left toe without noticing it is nonsense. Ryle accuses the traditional theory of being unable to provide a sensible account of the difference between a careful and a careless observer, but his own theory, while giving a plausible account of carefulness, fails to explain the activity of observing.

IX. THE DISPOSITIONAL THEORY RESTATED

Although Ryle has failed to provide a satisfactory account of consciousness, attention and observation in terms of dispositional theory of mental concepts, it would be unwise to conclude that such an account cannot be given. Ryle's account fails mainly because he overlooks the fact that our own activity is not the only sort of thing to which we can pay attention. The possibility of providing a plausible dispositional theory which takes account of our consciousness of and attention to objects, phenomena and sensations is not ruled out. Indeed it is not difficult to suggest the form which such a theory might take.

We have seen that although paying attention to what one is doing does not entail being prepared to meet the demands of the task in hand, it cannot be denied that to pay attention to something entails noticing and hence being able to say something about it. It must also, I think, be conceded that it involves being ready to encounter something, although one need not be prepared to encounter the sort of thing that is actually there. This, however, cannot be all that we are saying when we say that someone is attending to something, since we can be ready to behave in a manner appropriate to the presence of some object or event in our immediate environment without actually being conscious of it. Our disposition to act in this way may be a result of something we have been told, some inference we have drawn or some observation made a few moments previously. In such cases we might be said to know, remember or suspect that it was there, but we would not be observing, attending to or conscious of it. In order for us to be conscious of something our disposition to react to its presence must result from its impingement on our sense organs at the time.

With the qualification that the disposition must result from sensory stimulation, it becomes quite plausible to maintain that to be conscious of something is to be ready to

react both verbally and otherwise to the presence of some object or event in one's immediate environment. On this theory the contribution of attending to skilled performance would be explained by pointing out that unless the individual is disposed to react in a manner appropriate to the presence of the relevant features of his own activity and the environmental situation in which it takes place, he is not likely to be very successful.

On this view 'consciousness', 'attention' and 'observation' refer to a temporary state of readiness for something. You would therefore expect them to exhibit the logical features of expressions referring to temporary states of affairs. Expressions like 'being conscious' or 'aware of' do exhibit these logical characteristics. Words like 'attending', 'observing', 'watching', 'looking' and 'listening', on the other hand, exhibit the logical behaviour characteristic of expressions which refer to activities. This fact as we have seen, appears to provide a formidable obstacle to any dispositional theory of the meaning of these words. Nevertheless the difficulty can probably be overcome without appealing to any kind of internal process or activity, by examining the notion of 'activity' itself. It is at least arguable that when we speak of an individual's activities, of the things he does, we refer to those changes in him which can be induced by such things as commands, entreaties, instructions and deliberations. Any changes whether muscular or non-muscular which he can decide or be asked to bring about in himself are things which he does. Paying attention and observing are not muscular movements, nor are they movements of a mysterious transcendental musculature, they are, so the theory might run, changes in the individual's short term dispositions, readinesses or sets (to use a term which has a wide currency in the psychological literature) which can be induced by appropriate commands, requests or by decisions on the part of the individual himself.

In the light of these considerations we may restate the dispositional theory of attention, observation and consciousness as follows: to observe or pay attention to something is to bring about a change in oneself such that the impingement of the object or phenomenon in question on one's receptor organs prepares one to respond both verbally and otherwise in a manner appropriate to the presence of something; while to be conscious of something is to be so disposed.

X. THE CASE FOR THE TRADITIONAL VIEW

Stated in this way my quarrel with the dispositional theory is less substantial than my agreement with it. My contention is not so much that it is wrong as that it is incomplete. It is incomplete because it makes no reference to the internal state of the individual which enables him to describe and respond appropriately to the presence of objects in his vicinity. On the view which I wish to defend, when we use what Ryle calls a 'heed concept', we are not merely referring to the disposition to respond in a manner appropriate to the presence of the thing in question and specifying how that disposition is brought into being, we are also referring to an internal state of the individual which is a necessary and sufficient condition of the presence of such a disposition. I shall now try to present arguments in support of this contention.

One of the major weaknesses of Ryle's account of mental concepts is, as he himself recognizes, his retention of the traditional extended use of the term sensation (ch. VII). He is compelled to retain this use in order to provide something—having a sensation—which the observer of an object can be said to do heedfully. One of the advantages of the revised form of the dispositional theory which I have stated is that it dispenses with the

necessity for this concession to the traditional misappropriation of mental concepts. But although it dispenses with the necessity of abusing the concept of sensation, it runs into serious difficulties when applied to those cases where we do speak of being conscious of or of attending to our sensations, i.e. in those cases where our state of consciousness results from interoceptive or proprioceptive stimulation or from the various twists and quirks of our sensory apparatus, rather than from the impingement on our sense organs of any specifiable state of affairs in our environment.

Suppose that having applied pressure to my eyeball, I am conscious of a sensation of light. According to the revised dispositional theory this means that I am disposed to react to the presence of something. But what is it that I am disposed to react to? It cannot be the pressure on my eyeball, since to be conscious of light sensations is not the same thing as being conscious of pressure applied to the eyeball. But it cannot be the presence of the sensation either. Sensations, as we have seen, do not exist independently of our consciousness of them. There are not two things, my sensation and my consciousness of it, in the way that there are two things, a penny and my consciousness of the penny. The occurrence of a sensation entails someone's consciousness of that sensation.

To be disposed to react to a sensation therefore would be to be disposed to react to one's consciousness of that sensation. In other words we now have an infinite regress of dispositions, instead of the infinite regress of ghostly operations which appears so frequently in Ryle's criticisms of the traditional theories. We might be tempted to meet this objection by supposing that to say that someone is conscious of a sensation of light is to say that he is temporarily disposed to react as he would normally do if there had been a flash of light. But to be disposed to react as if there were a flash of light, would be to believe or be tempted to believe that a flash had occurred; whereas it makes perfectly good sense to say that he was conscious of a vivid sensation of light, yet it never occurred to him for one moment to suppose that there had been any actual flash of light. In other words an individual's state of consciousness is something over and above any dispositions which it arouses in him.

An objection which applies to any attempt to give a dispositional amount of consciousness and attention is the objection that it always makes sense to ask the individual to describe what it is like to watch, listen, observe or be conscious of something, whereas it does not make sense to ask him what it is like to have a certain capacity or tendency. We can only describe what something is like if it is an object, situation or occurrence. We can describe, characterize or define such things as relationships, capacities and tendencies, but we cannot describe what they are like. We can describe what a car is like, but we cannot describe what its horse-power is like; we can describe what it is like for one billiard ball to strike another and propel it forward, but we cannot describe what the causal relationship is like; we can describe what it is like to swim or what it is like to realize that one can swim, but not what it is like to be able to swim; we may be able to describe what it is like to be told or call to mind the fact that whales are mammals, but we cannot describe what it is like to know or believe that they are. If to be conscious of something were merely to be disposed to react in some way, it should be logically impossible for us to describe what it is like to be conscious of something. In fact there is no logical impossibility here. We are continually describing what it is like to watch, look at, listen to or feel things.

It might be objected with some justification here that what we describe is not our

consciousness, but the things we are conscious of. As we have seen, part of what is meant by saying that someone is conscious of something is that he can say something about it. It is certainly true that when we describe some object in our environment of which we are conscious, our description is a description of the object itself, and not, as has sometimes been supposed, a description of our consciousness of that object. It is also true that we cannot describe the state of being conscious in abstraction from the things we are conscious of. But that does not mean that we do not on occasions describe our consciousness of things as distinct from describing the things themselves. When we say, to use a familiar example, that the penny looks elliptical when viewed at an angle, we are not describing the penny, nor are we describing the image which it projects on our retina; we are describing what it is like to look at a penny from that particular angle; we are saying that it is somehow like looking at an ellipse viewed full face. When we say this, moreover, we do not imply that we are disposed to act in a manner appropriate to its being an ellipse. The elliptical shape of the penny is not an optical or a psychological illusion (cf. Ryle's discussion of this problem pp. 216-18).

When we describe a state of consciousness, we usually do so by comparing being conscious of one thing with being conscious of another. Nevertheless there are one or two expressions like 'pleasant', 'unpleasant', 'vivid', 'dim', 'acute' and 'vague' which we apply to the states of consciousness themselves. These are somewhat unusual adjectives to apply to a state of readiness. Furthermore the difference between vividness and dimness, acuteness and vagueness is difficult to explain on a dispositional theory of consciousness. The only possible interpretation on such a theory is in terms of the appropriateness of the behaviour, for which one is prepared, to the presence of whatever it is one is conscious of. Acute consciousness, however, does not guarantee the appropriateness of the resulting behaviour. The statement 'his consciousness of his own ineptitude was so acute that he was unable to do anything about it', makes perfectly good sense. It also describes a situation with which some of us are only too painfully familiar. If we recognize that consciousness is some sort of internal state of the individual these discrepancies between the intensity of the individual's consciousness and the adequacy of the behaviour for which it prepares him, no longer constitute a problem.

Finally, there are considerations of a more general nature. If there were no decisive arguments either way, we should probably prefer the dispositional to the internal process theory of consciousness and attention on the grounds of parsimony. As against this must be set the fact that in every other case where verbs having 'activity' characteristics are involved, it has been found impossible to apply a purely dispositional analysis, and in at least one group of cases the reference to internal processes within the individual cannot seriously be denied. The cases I have in mind here are thinking (in the sense of thinking about or thinking to oneself), pondering, calculating, imagining, dreaming, visualizing and doing mental arithmetic. Ryle (p. 27) has made a strong case for the view, that when we talk about someone thinking (in the relevant sense), pondering, calculating, or imagining we are not asserting the occurrence of any internal process or activity. He contends that the activity referred to, although sometimes covert, as when it consists of visualizing or performing mental arithmetic, need not be so. It may equally well consist in some entirely overt performance such as drawing, talking out loud to oneself or playing a game of make-believe. To assert that someone is thinking or imagining does not discriminate between these two possibilities. This argument disposes or, at least appears to dispose of,

the view that words like 'thinking' and 'imagining' necessarily assert the occurrence of covert activities, but there is no suggestion that these are dispositional concepts. Nor is there any attempt to deny that thinking sometimes consists in a purely covert process or that expressions like 'dreaming', 'visualizing' and 'mental arithmetic' refer to such processes. If this is conceded with respect to 'dreaming', 'visualizing' and 'mental arithmetic', it is difficult in view of the weight of traditional and common-sense opinion and the lack of any positive evidence against it, to see why a similar concession should not be made with respect to 'attending', 'observing', 'watching', 'looking' and 'listening'.

Concepts like 'observing', 'watching', 'listening' and 'being conscious of' are, in fact, closely related to concepts like visualizing and dreaming in a way, which is extremely difficult to explain, if the former are regarded as dispositional concepts. For if we want to explain what sort of thing this business of visualizing or dreaming is, the answer which immediately suggests itself is to say that visualizing something is like watching it except that there is nothing there really and you don't have to have your eyes open. Now if to watch something is merely to bring about a change in oneself such that the impingement of the thing in question on one's eyes prepares one to respond both verbally and otherwise in a manner appropriate to there being something there, this explanation becomes completely unintelligible. Apart from the fact that both visualizing and watching are things which the individual can be said to do, it is exceedingly difficult on this theory to find anything which the two cases have in common. We cannot say that to visualize is to be disposed to act and speak as if there were something impinging on one's eyes when in fact there is not. Any one who is so disposed would be suffering from a visual hallucination, and although having a visual hallucination may be said to involve visualizing, we can visualize things perfectly well without being hallucinated. The similarity between visualizing something and watching it lies in the internal state of the individual which is brought into being, not in the behavioural dispositions which that state induces.

XI. CONCLUSIONS

If the above arguments prove what I think they prove, are we back where we started at the beginning of Ryle's inquiry? Do these arguments merely put the Ghost back into the Machine? I do not think so. So far as I am aware, the criticisms I have made of the dispositional theory apply only to the dispositional analysis of consciousness and heed concepts generally. The dispositional analysis of intelligence, knowledge, belief, motives and memory remains unaffected, except in so far as these concepts involve dispositions to pay attention to or become conscious of certain features of one's environment. Indeed, since Ryle himself appears to accept the view that words like 'watching', 'listening' and 'observing' entail a reference to a covert process of having sensations, it is only in the case of the heedful performance of muscular activities that the view which has been urged in this paper differs from the account which Ryle has given as far as recognizing a reference to covert states and processes is concerned. On Ryle's view, however, these processes are relatively unimportant; we learn to talk silently to ourselves in order not to disturb others; we could plan our course of action on paper, but it is often more convenient to do it in our heads. If, on the other hand, our very ability to describe and adapt our behaviour to the objects and phenomena which impinge on our sense organs, is dependent on a special state of affairs within ourselves, which can itself be described by the person in whom it occurs, the reference which is made to such a process in our use of expressions

like 'attending', 'observing' and 'being conscious' can hardly be brushed aside as a matter of no great significance. If such a view is accepted, we can hardly avoid raising the question which Ryle has dodged persistently throughout his book, namely the question: 'What are these curious occurrences within ourselves on which we can give a running commentary as they occur?' Lack of space unfortunately precludes any discussion of this fascinating problem here. It is my belief, however, that the logical objections to the statement 'consciousness is a process in the brain' are no greater than the logical objections which might be raised to the statement 'lightning is a motion of electric charges'.

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AN EXPERIMENTAL STUDY OF HUMAN CURIOSITY

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I. INTRODUCTION

A drive which is aroused by a question and reduced by rehearsing its answer is identifiable with what we have called 'epistemic curiosity' (Berlyne, 1954). The present paper reports an experiment intended to test some of the predictions derivable from the theory that was built round this concept.

When we set out to inquire into a complex form of human motivation like curiosity, we find ourselves faced with a bewildering array of variables that may be relevant. The difficulty of knowing where to begin is, no doubt, one reason why little work in this area has been done. One indispensable aid is to have a theory (Hebb, 1951; Miller, 1951) to suggest relationships that are likely to repay investigation. But even then, the task may still seem baffling. There may be a vast network of factors involved, each making a comparatively slight contribution, and individual differences must be enormous. It seems therefore desirable to pass through an intermediate stage, if the project is to be practicable, namely an exploratory experiment. This would use small samples of subjects and sound the effects of several variables at once. It may well prove too insensitive to permit definitive conclusions about some of the relationships it studies. But it can save us from many a costly blind alley by confirming that certain lines of research are worth pursuing. The experiment to be reported below was designed for such an exploratory function.

II. PREDICTIONS FROM THE THEORY

The theory with which we are concerned (Berlyne, 1954) describes certain postulated, unobservable central processes. It is therefore necessary to relate our intervening variables to antecedent and consequent variables, in order that the empirical content of the theory may be revealed.

Epistemic curiosity, according to the theory, is aroused by 'thematic probes' (Skinner, 1947, 1953). The clearest examples of these are questions, which are assumed to evoke, among others, drive-producing meaning-responses (r_{mD}). The curiosity-drive-strength is assumed to increase with (1) the intensity of the drive-stimulus (s_{mD}) produced by the r_{mD} , and (2) the degree of conflict (F) between the symbolic meaning-responses.

The intensity of the s_{mD} will depend on the prolongation (extinction-resistance, n), and the strength (amplitude, A) of the drive-producing response (r_{mD}). These quantities, according to Hull (1943, 1952), depend on reaction-potential (sE_R), which is itself largely a function of habit-strength (sH_R), inhibitory aggregate ($\dot{I}_R$) and drive (D). To take these in turn, sH_R will be a function of the number of reinforcements (N), which in this case will mean the number of times that arousal of curiosity by similar questions in the past has led to a successful quest for an answer, with consequent reward, social or material.

Then, $\dot{I}_R$ will depend on the difficulty of finding an answer to similar questions in the past (amount of effort, W) and the number of times the quest has led to failure (n). As for D , Hull postulates that any drives present at the time will help to swell this variable, but in our case it is likely that irrelevant drives like hunger or thirst will evoke their own distracting responses more strongly than they will increase curiosity. But drives whose relief has on previous occasions been furthered by finding answers to comparable questions may well add their quota. They may include practical purposes for which the knowledge sought would be of use, social motives, e.g. drives for prestige and approval, and possibly such drives as Freudians mention, acting by displacement (Miller, 1948). The other variables on which ${}_SE_R$ is assumed to depend are likely to play a minor role in curiosity, but they may possibly not be without influence. They are J , or the speed with which reward has followed a successful inquiry in the past (this is probably of little importance because secondary rewards or 'confirmatory reactions' are likely to be immediate) and V (stimulus-intensity dynamism), which may represent the effect of the loudness, if spoken, or size and brightness, if written, of the question.

But the more innovative aspect of the theory, and the one with which the experiment is most closely concerned, is that which deals with the role of conflict (F). A discussion of the general design of the experiment will show how its effects may be studied empirically.

An experimental group received a fore-questionnaire about invertebrate animals, a communication providing answers to the questions and an after-questionnaire repeating the questions. A control group was given the communication and the after-questionnaire but not the fore-questionnaire. In this situation the theory would predict several phenomena:

(1) *Effects of pre-questioning*

It would be expected that the experimental group, when they read one of the answers, would recall the corresponding question from the fore-questionnaire, and that curiosity would be re-aroused as a result. The rehearsal of the answer would reduce the curiosity-drive to a subliminal value, and this drive-reduction would reinforce the learning of the answer. No such process could occur with the control group, as they had no fore-questionnaire. Other sources of reinforcement might well be present, but the experimental group, having this extra drive-reduction, would learn more effectively. They would therefore recall more answers than the control group when the after-questionnaire was presented.

The arousal and subsequent reduction of curiosity is stated to increase, by stimulus generalization, the amplitude of curiosity-producing responses (r_{mD}) to similar questions in future. Consequently, when subjects are required to indicate which of the animals figuring in the experiment they would like to know more about, the experimental group can be expected to mark more animals than the control group.

(2) *Recognition of answers*

When one of the answers in the communication is received, curiosity aroused by the recall of the question will be reduced. If a question is recalled, its answer will presumably be recognized as such. It follows that the learning of statements which are recognized as answers to questions from the fore-questionnaire will have the reinforcement of this curiosity-reduction, and so they will be more probably recalled than others during the after-questionnaire.

(3) Effectiveness of different classes of questions

We have two ways of comparing the effectiveness in arousing curiosity of two different classes of questions. Subjects were instructed to mark with 'K' twelve questions out of the forty-eight in the fore-questionnaire whose answers they most wanted to know, and so we can note the number of questions in each class that were marked 'K'. Secondly, we can note the number of questions in each class that were answered correctly during the after-questionnaire.

The conflict-variable (F), which the theory cites as a determinant of strength of curiosity, is stated to depend on the equality, the absolute strength, the number and the degree of incompatibility of the competing response-tendencies. This allows us to identify certain classes of questions which can be expected to arouse more curiosity than others, as shown by both ways of gauging curiosity.

Familiarity. It is hypothesized that the more familiar the principal concepts in the questions, the more numerous and the stronger the competing symbolic response-sequences to which they give rise. Noble (1952, 1953) has demonstrated that there is a correlation between the familiarity of words and the number of associations they elicit. Subjects were instructed to rate the animals figuring in the questions for familiarity at the start of the experiment, and familiarity was also manipulated by providing introductory information about two of the animals. Questions about more familiar animals, familiarity being judged by either operation, should arouse more curiosity than others.

Incompatibility. Questions whose concepts seem incompatible to subjects will arouse more conflict than others and consequently more curiosity. Incompatibility was judged by having subjects mark those questions in the fore-questionnaire which surprised them, and also by using a group of judges, who indicated which predicates seemed to them least applicable to the animals.

A preliminary experiment, reported elsewhere (Berlyne, 1953, appendix), supported the prediction that questions with four alternative answers aroused more curiosity than those with two alternative answers, as we should expect in view of the number-of-competing-tendencies variable.

(4) Correlation between measures of curiosity

The questions which subjects marked with 'K', as arousing most curiosity in the fore-questionnaire, should be more likely than others to be answered correctly in the after-questionnaire. There are two mechanisms by which this could occur. Whether a question is marked 'K' depends on how much curiosity it arouses in the fore-questionnaire, and whether it is answered correctly in the after-questionnaire is assumed to depend on the strength of curiosity at the time of reading the answer in the communication. The theory implies that these two degrees of curiosity will be correlated. A supplementary or alternative mechanism might be a closer reading of questions selected for marking with 'K' and consequently a more probable recall of such questions in the communication. Either mechanism would cause our two measures to be correlated and would make both depend on the degree of curiosity aroused by the questions.

(5) Surprisingness of answers

Subjects were instructed to indicate which statements in the communication they found surprising. Such statements will have some apparent incompatibility between their

concepts, and this will add to the curiosity aroused and then reduced. These statements should therefore be more effectively learned and more probably recalled in the after-questionnaire than others.

There were sixty-eight subjects and thirty judges; all were undergraduates taking a course in Normal Human Personality at Brooklyn College. All had had a previous introductory course in General Psychology. Subjects occupying alternate seats were allotted to the experimental group and to the control group.

III. THE EXPERIMENT

SUBJECTS. *Experimental Group* (thirty-four subjects).

The following three phases were taken in immediate succession.

Phase I:

Section 1. A list of twelve animals was presented, and subjects were instructed to rate each animal for familiarity on a 4-point scale, ranging from 'seen, read or heard a fair amount of' to 'never heard of'. The twelve animals consisted of eight that subjects were almost bound to have heard of and four 'exotic' animals that were unlikely to be known to them. Of the 'exotic' animals, two were real (the sea-mat and the sea-gooseberry) and two were fictitious (the 'sea-wasp' and the 'stringworm').

Section 2. Paragraphs of about 120 words each, giving a general description of two of the 'exotic' animals, one real and one fictitious, were read by subject. For half the subjects (sub-group EA) the animals were the sea-gooseberry and the 'stringworm', while for the remainder (sub-group EB) they were the sea-mat and the 'sea-wasp'.

Section 3. A randomized series of 48 questions, 4 on each animal, was presented, and subject had (a) to check off the correct answers from two alternatives per question, (b) to mark with 'C' those answers of which he felt certain from previous knowledge, and (c) to mark with 'S' those questions which surprised him.

Section 4. Subject had to go back over the questions in section 3 and mark with 'K' the three questions out of each consecutive set of twelve whose answers he would most like to know. Twelve questions out of forty-eight were thus to be so marked.

Phase II:

Subject received sheets containing seventy-two statements, six about each of the twelve animals. These were in a random order, not corresponding to the random order of the questions in Phase I. Subject was (a) to mark with 'S' every statement that surprised him, and (b) to tick every statement that he recognized as an answer to one of the questions in Phase I. An answer to each of the forty-eight questions, coinciding with one of the alternatives offered in Phase I, was included among the seventy-two statements. The tasks served the double purpose of providing the experimenter with information and inducing, as far as possible, a non-learning set.

Phase III:

Section 1. The forty-eight critical questions were again presented, this time in an open-ended form and in yet a third random order. Subject had to fill in the answer given in Phase II, when remembered.

Section 2. The twelve animals were listed, and subject had to check off those he would like to know more about. Finally, there were questions on the interestingness of the experiment and desire to know more about animals.

Control Group (thirty-four subjects).

The procedure was exactly the same as for the experimental group, except that sections 3 and 4 of Phase I (the fore-questionnaire) were omitted, and the recognition-test in Phase II did not apply. Phase I existed in two versions, as with the experimental group: seventeen subjects (sub-group CA) received paragraphs about the sea-gooseberry and the 'stringworm', and the other seventeen (sub-group CB) read about the sea-mat and the 'sea-wasp'.

JUDGES (thirty).

The judges, taken from the same population as the subjects, received a list of the eight 'non-exotic' animals used in the experiment. Opposite the name of each animal were four phrases, representing the predicates implied by each of the four questions about that animal. The judges had to check off the two phrases that seemed to them least likely to apply to the animal concerned.

Of the forty-eight answers to questions from the fore-questionnaire that were presented in Phase II, twenty-four (two per animal) were true and twenty-four (two per animal) were false. This was to control for previous knowledge of the subject-matter and for the fact that the animals mentioned differed in familiarity. If all the answers provided had been true, more knowledgeable subjects would have had more coincidences between the answers checked in Phase I and those learned in Phase II, and the following possibilities might have invalidated the conclusions:

(1) Any previous knowledge that inclined a subject to prefer a particular response in Phase I may have made the corresponding fact easier to learn in Phase II.

(2) The act of selecting an answer in Phase I may have provided reinforcement for the bond between question and answer to supplement the learning in Phase II.

The familiarity-variable necessitated precautions against two further possibilities:

(1) The correct answers to questions about familiar animals might have been encountered in the past. These answers might have been subliminal, so that they could not be recalled in Phase I, but they might nevertheless have provided a residue of habit-strength to make the re-learning of them in Phase II easier.

(2) It might have been possible to infer the correct answer from what was already known about the animals. (We guarded against this also by the inclusion of fictitious animals, the answers being selected at random in their case.)

The expedient of making half the answers false ensured that, no matter how much a subject's prior knowledge about the animals, this knowledge would facilitate and hinder the learning of equal numbers of answers. It caused the proportion of coincidences per subject between answers checked in Phase I and answers learned in Phase II to approximate one-half.

IV. RESULTS AND DISCUSSION

Computing the results

The following rules were observed in computing the results, except where stated otherwise.

(1) All questions marked 'C' in the fore-questionnaire (i.e. those whose answers subjects claimed to know with certainty) were omitted. This was because in such cases curiosity may not be aroused at all or may function in a special way, and different subjects felt certain of different numbers of answers.

(2) In some cases, subjects appeared to have misunderstood or overlooked some of the tasks. Therefore, (a) if subject had marked no question with 'S' (to indicate surprise) in Phase I, his data were omitted from all calculations involving that variable, (b) if subject marked all statements in Phase II with either 'S' (indicating surprise) or with a tick (indicating recognition of an answer), but not both, his data were disregarded where these variables were relevant, (c) if subject marked more or less than twelve of the questions in Phase I with 'K' (indicating desire to know the answer) his data were included, but if he marked none at all they were disregarded where relevant.

For the comparisons between experimental and control groups, we used the t -test. But for other results, which involve comparisons between questions or answers, differing in number from subject to subject, that belonged to different categories, we used the χ^2 -test for data pooled from all subjects, with Yates's correction. Some statisticians object to the use of the χ^2 for such data, one objection being that the test does not distinguish between effects which the group as a whole has produced and those due largely to a few exceptional individuals. We have consequently given, in columns headed 'Ss' in the tables, the number of subjects whose data show a deviation from expected proportions in the same direction as the contingency table for the whole group. For example, 26/29 indicates that twenty-six out of twenty-nine subjects show such a deviation. This enables us to supplement the χ^2 analysis with the sign-test (Moses, 1952). Our experiment, having mainly an exploratory function, is unfortunately not sensitive enough for such techniques as the analysis of variance for proportions based on unequal numbers (Cochran, 1943).

Effects of pre-questioning. Table 1 displays the comparisons between experimental and control group that show whether the pre-questioning, which only the experimental group underwent, increased curiosity. We compare the mean numbers of questions answered correctly in Phase III and also the mean numbers of animals ticked in section 2, Phase III. Both differences confirm the prediction significantly. All questions, including those marked 'C' are included. No significant differences were produced by any of the questions on interest, etc.

In view of the fact that the experimental group had two tasks to carry out in Phase II and the control group had only one, it might be thought that some difference in the thoroughness of reading the statements might have been responsible for the greater recall-scores of the experimental group. But a similar significant difference was found in the preliminary experiment (Berlyne, 1953, appendix), in which both groups alike had merely to read through the communication in Phase II.

Table 1. *Effects of pre-questioning*

Mean correct answers in phase III		t	P	Mean animals ticked in Phase III		t	P
Exp. group	Cont. group			Exp. group	Cont. group		
32.41	27.15	3.29	<0.01	5.38	3.36	2.90	<0.01

Recognition of answers. The first row of Table 2 shows that statements recognized by the experimental group as answers to questions from Phase I were, as predicted, more likely than others to be recalled in the after-questionnaire. The second row of the table shows that questions subjects marked 'K' (i.e. those whose answers they most wished to know) in Phase I were more likely to have their answers recognized in Phase II. The latter relationship was not predicted by the theory. To explain it, we can only hazard the

Table 2. *Relations between recognition of answer and measures of curiosity*

Questions	Answers		χ^2	P	Ss
	Answers recognized	Answers not recognized			
Answered correctly in Phase III	732	72	31.38	<0.0001	26/29**
Not answered correctly	325	91	—	—	—
Marked K	256	24	7.25	<0.01	21.5/27**
Not marked K	780	138	—	—	—

** Significant at 0.01 level by sign-test.

hypotheses that (1) questions evoking more curiosity may, like other drive-arousing stimuli, receive more attention (Berlyne, 1951, p. 143) and thus be more easily remembered, (2) questions marked 'K' may receive more attention, or (3) the drive-producing responses, though subliminal, may persist in some form, so as to supplement the statements in Phase II as stimuli re-evoking the questions. Such subliminal drives, known as 'quasi-needs' or 'tensions', have been posited by Lewin and his followers (1935) as an explanation of the Zeigarnik effect. They are also reminiscent of Freud's 'unconscious wishes', which may likewise lie dormant until they determine a response jointly with incoming external stimuli.

Effectiveness of different classes of questions. The last four rows of Table 3 present comparisons between different classes of questions put to the experimental group according to our two ways of estimating the effectiveness with which they arouse curiosity. We examine the number of questions in each class answered correctly and the number answered incorrectly in Phase III, and similarly we compare the numbers marked 'K' (as among the twelve whose answers subjects most wished to know) and not marked 'K' in Phase I.

Familiarity. We compare questions about animals marked 'seen, read or heard a fair amount' or 'seen, read or heard a little' (hereafter referred to as the '*more familiar animals*') with those marked 'heard of, but know nothing about' or 'never heard of' (hereafter referred to as the '*less familiar animals*'). Table 3 shows that questions about more familiar animals evoked more curiosity by both tests. The two 'exotic' animals

Table 3. *Effectiveness of different classes of questions*

(N.S. = 'not significant')

Classes of questions	Correct in Phase III	Not correct in Phase III	χ^2	P	Ss	Marked		Not marked		χ^2	P	Ss
						K		K				
Marked K	251	76	20.11	<0.0001	28/31**	—	—	—	—	—	—	—
Not marked K	670	390	—	—	—	—	—	—	—	—	—	—
More familiar animals	659	239	44.50	<0.0001	29/34**	259	535	36.70	<0.0001	26/32**	—	—
Less familiar animals	187	163	—	—	—	50	286	—	—	—	—	—
Previous-information 'exotic' animals	149	111	NS	—	—	28	213	NS	—	—	—	—
No-previous-informa- tion 'exotic' animals	138	135	—	—	—	23	225	—	—	—	—	—
Surprising	153	88	NS	—	—	81	130	20.11	<0.0001	23/28**	—	—
Non-surprising	751	411	—	—	—	230	761	—	—	—	—	—
Incompatible concepts	362	159	NS	—	—	169	293	6.98	<0.01	24.5/31	—	—
Non-incompatible concepts	355	140	—	—	—	131	329	—	—	—	—	—

** Significant at 0.01 level by sign-test.

about which subjects received previous information were omitted from this comparison, as they received a special treatment. The other two were included, except where a fictitious animal received a rating other than 'never heard of'.

It might be suggested that the better recall-scores for questions about more familiar animals were due to the fact that more of these questions were marked 'K', which may have caused them to be read more attentively in Phase I and perhaps remembered more easily in Phase II. However, in the preliminary experiment (Berlyne, 1953, appendix), only half the subjects in the experimental group had to carry out the task of marking questions with 'K', and no significant effects on recall-scores of the presence or absence of this operation were found.

Table 4. *Recall of different classes of statements*

(NS = 'not significant')

Group	Classes of statements	Recalled in Phase III	Not recalled in Phase III	χ^2	<i>P</i>	Ss
Cont.	More familiar animals	498	336	24.50	<0.0001	26.5/34**
	Less familiar animals	178	223	—	—	—
Cont.	Previous-in-formation 'exotic' animals	127	135	6.33	<0.05	22.5/34
	No-previous information 'exotic' animals	102	169	—	—	—
Cont.	Surprising	369	292	NS	—	—
	Non-surprising	438	392	—	—	—
Exp.	Surprising	188	75	5.06	<0.05	21/29*
	Non-surprising	662	375	—	—	—

* Significant at 0.05 level by sign-test.

** Significant at 0.01 level by sign-test.

The effects of *questions* about familiar animals must be distinguished from any tendency there might be to remember *facts* about familiar animals better. The data on the control group in Table 4 reveal that there is such a tendency. However, we can ascertain whether questions about more familiar animals have an effect over and above this by comparing for the two groups the proportions of *questions answered correctly* in Phase III that fell in the two categories. We already know that pre-questioning increases the probability of recalling facts. But we wish to know whether this increase is significantly greater for questions about more familiar animals, i.e. whether such questions arouse more curiosity. If they do not, we should expect the correctly answered questions to be distributed in equal proportions between more familiar and less familiar animals for both groups. Table 5 supports the prediction that more curiosity is aroused by questions relating to more familiar animals. In the case of the control group, the figures are adjusted, because those for the experimental group excluded questions marked 'C' (those whose answers subjects claimed to be certain of). It is assumed that the control group would have marked with 'C' the same proportions of questions in each category as the experimental group, and the totals are reduced by these proportions.

Each of the four '*exotic*' animals received an introductory paragraph of information for half the subjects of each group, but not for the other half. We are thus enabled,

confining our attention to the 'exotic' animals alone, to compare the effects of questions about *previous-information* and *non-previous information* animals respectively. Table 3 shows that there is no significant difference in curiosity by either test. But the data from the control group in Table 4 (adjusted, as above) suggest there is a tendency for facts about previous-information animals to be learned better. The failure of a significant difference to appear for the experimental group may well have been due to its being swamped by the pre-questioning variable, which may have produced a ceiling-effect.

Table 5. *Questions answered correctly in Phase III*

Group	More familiar animals	Less familiar animals	χ^2	<i>P</i>
Exp.	659	187	3.46	<0.05 (1 tail)
Cont.	498	178	—	—

Incompatibility. There are two indications available to us of which questions contradicted subjects' expectations. We have the marking of questions with 'S' in Phase I, which reveals those found *surprising*, and we have the judges' selection of the two questions out of the four on each animal that involved what appeared to them the least applicable predicates. The latter dichotomy between '*incompatible-concept*' and '*non-incompatible-concept*' questions has the advantage of being orthogonal to any comparisons between animals on the grounds of familiarity, etc., as each animal had two questions in each category. Table 3 shows that, according subjects' reports (marking 'K') of desire to know answers, surprising and incompatible-concept questions evoked more curiosity than the others. But no effect of these variables on recall was manifested. We can check on the agreement among the judges in their selection of incompatible concepts by counting the number of judges marking the two most and the number marking the two least frequently selected concepts for each animal, and performing a mean-difference test. The mean difference per animal is 14.52, $t=4.09$ and *P* is less than 0.01. The preliminary experiment (Berlyne, 1953, appendix) supplied some slight evidence in favour of a correlation between surprise and probability of recall, and so lack of sensitivity may have been responsible for the failure of this effect to reveal itself.

Correlation between measures of curiosity. The first row in Table 3 confirms that questions marked 'K' by the experimental group in Phase I, indicating desire to know the answers, were more likely to be answered correctly in Phase III, thus revealing the predicted correlation between our two measures of curiosity. Since we find that both the recognition and the subsequent recall of answers are more probable if the corresponding questions are marked 'K', the relation between the first two may be a reflection of these correlations instead of being due to the reasons we adduced. The present experiment alone does not allow us to decide between these alternative explanations.

Surprisingness of answers. It remains to investigate the comparative ease of learning statements marked 'S' (as surprising) in Phase II and statements not marked 'S'. The last two rows of Table 4 show that the experimental group, as predicted, recalled a higher proportion of surprising statements in Phase III. But this prediction was not significantly borne out by the control group.

No legitimate conclusion can be drawn from this difference between the two groups, as the control group marked significantly more statements as surprising ($\chi^2=237.94$, *P* is less than 0.0001). This was probably due to the fact that the control group had only one

task to perform in Phase II, whereas the experimental group had two. As a result, subjects in the control group are likely to have had a lower threshold of surprisingness for a statement to be marked 'S', and so their data may have been less sharply discriminating.

The effect of the surprisingness of an answer could be explained away if this variable were associated with the familiarity of the animals. But χ^2 for both experimental and control groups is under 1.00, and so this can be ruled out.

V. SUMMARY

An exploratory experiment, designed to test some of the predictions from a theory of human epistemic curiosity, is reported. An experimental group received (1) a fore-questionnaire about invertebrate animals, (2) a series of statements including answers to the questions, and (3) an after-questionnaire, repeating the questions of the fore-questionnaire. A control group underwent the same procedure except for the fore-questionnaire. The recall of the answers in the after-questionnaire and subjects' reports were used as measures of curiosity. The results tend to confirm the following predictions: (1) that pre-questioning arouses curiosity, (2) that the two measures of curiosity are correlated, (3) that statements recognized as answers to questions from the fore-questionnaire are more likely than others to be recalled in the after-questionnaire, (4) that questions about more familiar animals and questions whose concepts seem incompatible arouse more curiosity than others, (5) that surprising statements are more likely to be recalled as answers in the after-questionnaire than others.

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BETWEEN WAKEFULNESS AND SLEEP: HYPNAGOGIC IMAGERY

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I. *Introduction* (pp. 266–267). II. *Plan of present investigation* (pp. 267–268): (i) *Pilot-study* (pp. 267–268); (ii) *Main investigation* (p. 268). III. *Data obtained* (pp. 268–271). IV. *Discussion with further illustrative instances* (pp. 271–273). V. *Notes towards a theory of visual hypnagogic images* (pp. 273–275). VI. *Summary* (pp. 275–276). *References* (p. 276).

I. INTRODUCTION

Interesting and little-understood psychological states divide wakefulness from total sleep. These ‘twilight’ or ‘hypnagogic’ states are sometimes accompanied by imagery which in vividness and other characteristics appears different from that of both dreams and ordinary waking fantasy. In his *Dictionary of Psychology* Warren defines hypnagogic imagery as ‘imagery of any sense modality, frequently of almost hallucinatory character, which is experienced in the drowsy state preceding deep sleep’.

To illustrate such imagery we shall quote the experience of one of the subjects of the investigation to be reported: ‘I had images of bright green frogs which appeared to leer at me, then when they blinked their eyes, they vanished. Once I had images of evil-looking faces peeping over their shoulders at me.’ (Female, aged 18 years 8 months.) Another subject’s report illustrates equivalent auditory imagery: ‘Sometimes I hear bells before I sleep, loud bells and tinkling bells, all ringing together.’ (Female, 18 years 5 months.) Whether we call such experiences ‘images’, ‘visions’ (Leaning, 1925), or ‘hallucinations’ (Freud, 1900), our findings support the view stated by Leaning that the experiences are ‘common’, and do not accord with the low incidence found by Müller (1848), also working on a student sample. Frequent, regular hypnagogic imagery seems relatively rare, but to have experienced at least one hypnagogic image seems to be by no means uncommon. Some individuals nevertheless are unable to recall ever having had such images, and to these people a hypnagogic imager may have great difficulty in communicating his own experiences. This difficulty is discussed by Galton (1883) who, in his inquiries into visual memory, was impressed by the number of people who reported ‘visions’. Galton draws attention to the intolerance which the individual with atypical, or seemingly atypical, visualizing experiences is likely to encounter at the hands of other people.

Hypnagogic images are interesting in themselves, but seem also to be relevant to a number of wider issues. Certain writers, among them Robert Louis Stevenson (Still, 1952, p. 192), and probably Edgar Allan Poe (Mowrer, 1950, p. 635n.), appear to have used their hypnagogic imagery as sources of their literary productions. The seemingly ‘original’ characteristics of at least some hypnagogic images may make them relevant to the understanding of certain kinds of creative thinking. Again, certain ‘visions’ of a kind which have been, and still are, interpreted supernaturally resemble closely what might be classified under the more neutral heading of hypnagogic images. Hobbes (1651, ch. xii), Müller (1848), and Ernest Jones (1949, pp. 58f.) have been among those who have drawn

attention to the ways in which dreams and visionary experiences have contributed to the world's superstitions and folklore. The history of hypnagogic images, interpreted supernaturally as 'visions', 'omens', and 'precognitions', rather than naturalistically, is probably a long one. If Skinner's view (1953) of superstition as a product of the reinforcement of accidental contingencies is helpful, the chance occurrence of a vivid hypnagogic image with an emotionally significant event would be a potent source for superstition. It is probable that the importance of hypnagogic images in this context has been underestimated rather than overestimated. Certainly a hypnagogic image when it is experienced can mean very different things to different people. To a psychologist it may be simply an interesting phenomenon for study; to a writer or painter it may be valuable subject-matter for use; to a normal adolescent or a person who fears 'insanity' it may be a source of worry or even terror. Since hypnagogic images have resemblance to both 'visions' and hallucinations, it is relevant to record that our own interest in them arose during investigations with the drug mescaline, which is commonly used experimentally to induce psychotic-like states. While experimentally under the influence of mescaline, the first writer sought to communicate his visual experiences by searching for the closest analogy he could in experiences that the experimenters might have had. He found this analogy in hypnagogic imagery, and has since learnt that the resemblance of mescaline visions to hypnagogic images has also been independently noted by Rawcliffe (1952). Precisely because the two judgements are entirely independent, this agreement is the more interesting. If the resemblance is a true one, hypnagogic images have additional interest, since mescaline has had two main uses: (a) the induction of visions for mystical and religious purposes, first by the Aztecs and later by contemporary Central American Indian tribes, and, (b) the induction of psychotic-like states and accompanying hallucinations by experimental investigators of schizophrenia. It is interesting to consider the hypnagogic image in relation to the continuum which Galton postulates between complete hallucination and the more normal forms of imagery, particularly since, as Osmond & Smythies have recently pointed out (1952), the biochemistry of mescaline and adrenaline is very similar. Hypnagogic images merit investigation in that they may represent an instance in which greater knowledge of the 'normal' may illuminate the 'abnormal'.

It will be our task to describe instances of such images, to provide a provisional classification and tentative quantitative data, and to consider their possible relevance to the broader issues that we have indicated. We have attempted to distinguish what might legitimately be called 'hypnagogic images' from other related or superficially similar phenomena. The latter include ordinary images, dream images, hallucinations, the images that carry over from sleep into wakefulness (hypnopompic images), and the images of those to whom nitrous oxide or certain other general anaesthetics have been administered.

II. PLAN OF PRESENT INVESTIGATION

(i) *Pilot-study*¹

The main investigation followed a questionnaire study of imagery, and related processes, conducted in 1952 on a group of Aberdeen University students, 28 male and 52 female, whose mean age was 21 years. Although the primary purpose of the questionnaire was not to elicit detailed information about hypnagogic imagery, five questions relating to this

¹ Reported in a paper given to the Scottish Branch of the British Psychological Society, Aberdeen, 9 May 1953. Copies of our questionnaires are lodged with the Technical Information and Documents Unit, D.S.I.R.

subject were included, and it was on the basis of the answers obtained that the investigators were able to design the more suitable questionnaire used in the main investigation. Despite the inadequacy of the questions, no less than 50% of the subjects reported some kind of hypnagogic imagery, the commonest being visual, though this may well have been an artefact of a visual bias in the wording of the questionnaire. Further preliminary studies were conducted on colleagues and advanced students, who were found to have an even higher incidence of hypnagogic imagery. Rejecting the improbable hypothesis that mental abnormality is unusually high among their students and colleagues, the writers approached the main investigation with the implicit assumption that they were studying a 'normal' rather than 'abnormal' psychological phenomenon.

(ii) *Main investigation*

The aims of the investigation were to obtain descriptions of a reasonably large number of instances of hypnagogic images of different types, to look for regularities in their content and structure, and to classify them. We have been concerned more with qualitative description than with frequencies of incidence. Two groups were investigated:

Group A. Students of Aberdeen University, 110 in all, 50 male and 60 female. This group was the subject of the more 'extensive' part of the study (mean age 20 years 11 months).

Group B. A heterogeneous group of colleagues; fluent hypnagogic imagers; post-graduate students and other adults ($N=30$). All may safely be assessed as of above average intelligence, and their introspections tended to possess both the merits and demerits of 'introspective sophistication'. This group was the subject of the more 'intensive' part of the investigations.

All figures quoted relate to group A, and not to group B: illustrative material is taken from both.

The questionnaire related primarily to hypnagogic imagery, but questions relating to other possibly relevant experiences were also asked; these included *déjà vu*, colour associations, number forms, the pre-sleep experience of 'falling', etc. It was thought justifiable to inform subjects that what was being investigated was an experience undergone by a large number of people who were not apparently maladjusted or psychotic. Despite this, it seems likely that superficial investigation by questionnaire is more likely to underestimate than to overestimate the incidence of such experiences. While reassurance may greatly minimize conscious resistance, it can do relatively little to prevent repression or 'denial' (in the psychoanalytic sense) of hypnagogic images. Information on this and on other matters of detail was furnished by group B, in interview, and by some of the subjects in group A, after they had completed the questionnaire.

III. DATA OBTAINED

Incidence. Hypnagogic imagery was reported by 67 of our 110 subjects (27 male and 40 female). For comparative purposes we may note that it was more common than colour associations (28 instances); hypnopompic images (23 instances); synaesthesia (15 instances); and number forms (8 instances). It was less common than *déjà vu* (78 instances) and the before sleep experience of 'falling and waking up with a jerk' (88 instances).

Kinds of imagery. When the hypnagogic images of the 67 subjects who had them were

classified according to their sense-mode, the commonest were the auditory (50 instances), and the next most common the visual (39 instances). Kinaesthetic imagery was reported by 11 subjects, and tactile and olfactory imagery in one instance each. (One of the subjects in the pilot investigation reported a visual image with accompanying temperature imagery.) When the 67 subjects were classified according to the kinds of hypnagogic images that they had, 27 reported auditory images alone; 15 auditory and visual; 12, visual alone; 6, auditory, visual and kinaesthetic; 4, visual and kinaesthetic; 1, auditory and kinaesthetic; 1, visual and olfactory; and 1, visual, auditory and tactile. It will be noted that 39 subjects had only one kind of hypnagogic imagery, 21 subjects had two kinds, and 7 had three kinds.

Auditory imagery. Of the 50 subjects who reported auditory hypnagogic imagery, 2 described them as occurring 'regularly', 12 as occurring 'often', and 36 reported them 'occasionally'. Auditory images of music were the commonest kind, and were quite often extremely specific. Thus one subject reported: 'The most common is the last movement of Rachmaninov's Second Piano Concerto. Occasionally I hear the First Movement of his Third Concerto, which I do not know well enough to construct in my mind when I am fully conscious.' (Male, 18 years 8 months.) In 17 of the 50 instances the imagery was confused with real sounds. The following report is typical: 'I once thought I heard my mother calling me just as I was dropping off, but when I got up I found she had not.' (Female, 18 years 11 months.) This difficulty of distinguishing hypnagogic images from hallucinations was most apparent with the auditory kind. Because of the predominantly visual character of dreams the auditory images were readily distinguished from dream images.

Visual images. One of the 39 subjects who reported visual hypnagogic images had the experience 'regularly', 4 had the experience 'often', and 34 'occasionally'. The images were coloured in 18 instances, uncoloured in 14, and unstated in 7 cases. The vividness and other characteristics of the colour experiences seemed to impress greatly a number of the subjects. Visual hypnagogic images were ordinarily experienced with closed eyes, but open-eyed images have been reported (e.g. Rawcliffe, 1952). Our data confirm this, since in 6 cases subjects experienced them with open eyes. The open-eyed hypnagogic image of 'the room being full of angels' reported by one subject, particularly as it immediately preceded the death of her husband, is the type of experience likely to be given a supernatural interpretation, as it was in this instance. Other instances, however—for example, the open-eyed image of 'a ship in a storm—the mast was broken and the sails were torn—tossed by huge waves'—did not involve the kind of 'accidental contingency', which, following Skinner (1953), we have suggested would probably play an important part in supernatural interpretations of natural phenomena. It is very difficult to draw any sharp line between such open-eyed 'hypnagogic' experiences, 'eidetic' images, and actual 'hallucinations'. Whatever we call them these visualizations represent a sort of analogue to the auditory images which were confused with real perceptions.

The content of the visual hypnagogic images was extremely varied and such as to evoke a variety of emotional reactions. To one subject his images were 'a background to sleep, almost in fact a lullaby'; to another they were a source of amusement and pleasure, their content being rather like that of a Walt Disney cartoon; to others they were a source of acute distress and even terror, being, as one subject reported, 'a terrible jungle through which I have to pass before sleep comes'.

Differences from dreams. The subjects were asked to say in what ways their hypnagogic images differed from their dreams. Typical answers were that the images were more vivid and realistic, that they 'come and go in a flash', and that the subject did not participate in them as in a dream, but 'only watched'. One subject described his images as 'like a succession of vivid lantern slides appearing without voluntary control, and containing detailed material which I didn't know I knew'. (Male, 34 years 7 months.)

Subjects were also asked how they knew that they were awake and not asleep when experiencing the images. Since the question is a difficult one, several were unable to give a satisfactory answer, and there is always the possibility that hypnagogic images, particularly those of a visual kind, may be confused with those of dreams. Among the reasons given for believing oneself to be awake were: being able to have ordinary perceptions at the same time (the commonest reason given); being able to have separate thoughts; being able to engage in conversation; being able to open eyes, close them, and continue with the image, etc. The following instance is far from atypical in the definiteness with which the subject was aware of being awake: 'It was dark outside, but I "saw" out of the window—bright sky, coloured trees. I could see people sitting on the roof—what they were wearing and the features of their faces. I woke my sister who was sleeping beside me and said "Look, Ella!" and began to count the people.' (Female, 19 years 3 months.)

Differences from ordinary images. To the question in what way their hypnagogic images differed from ordinary images, two subjects reported no difference, and several were unable to respond. The majority, however, were able to put the difference into words. They described their hypnagogic images as being clearer and more vivid; less subject to conscious control; brighter in colour; 'more a train of events than a single image which can be called up in the mind, and one thing leads to the next'; 'I could feel I wasn't exactly doing the things'; and other reasons. Gordon (1949, 1950) has developed the idea, stemming from the work of Jaensch (1930) on eidetic images, that ordinary waking images can be divided into those which are 'autonomous', and those which are of a voluntary controlled kind. Hypnagogic images seem to be largely, if not exclusively, of the autonomous kind. At an early date Müller (1848) stressed the autonomy of his own hypnagogic images and the fact that he had no control over either their production or their changing form. The visual images closely resemble dreams in this characteristic, which, as Gordon's work shows, is also possessed by some but not all ordinary images.

Definition of hypnagogic images. Subjects were given this definition of hypnagogic images: 'Imagery of any sense, frequently of intense, almost hallucinatory vividness, experienced in the drowsy state before sleep.' They were asked to state whether this adequately described their own experiences, and if not in what way their images differed. Of the subjects, 33 agreed it was a good definition, 21 disagreed in some particular and 13 had no comment. The reason for the disagreement most commonly given was that the image was not as vivid as the definition implied (18 instances): 'never intense or hallucinatory', and 'not particularly vivid, but always extremely persistent' are representative comments. Other subjects supported the interesting observation made by Collard (1953) that drowsiness and subsequent sleep were not invariably necessary to the occurrence of the imagery. One subject reported: 'I can be in bed wide awake or even have them in the ordinary course of the day if alone.' There are probably wide individual differences in this matter, and our data yield definite evidence that in some cases

drowsiness is not a necessary component. Perhaps, however, these deviant cases are better described as 'eidetic' than as 'hypnagogic' images.

Conditions favourable to hypnagogic images. It will be noted that some subjects experienced hypnagogic images regularly and without any special precipitating factors. We should not wish to set much store by the subjects' statements about the causes of their images. As might be expected, words like 'worry' and 'fatigue' were frequently used, but these are highly ambiguous and likely to be merely rationalizations and stereotyped ways of responding to a question on a matter of this kind. Definite evidence regarding the mental conditions that favour hypnagogic imagery must await further investigation, which may disclose wide individual differences.

IV. DISCUSSION WITH FURTHER ILLUSTRATIVE INSTANCES

Readers interested in the history of investigations of hypnagogic imagery are referred to the scholarly study by Leaning (1925). Leaning, who draws attention to the lack of investigations by psychologists in this field, surveys alternative explanations and lends his own support to a supernatural rather than a naturalistic theory. Any alternative naturalistic explanation, such as we should look for, must do justice to the fact that imagers often find the experiences extremely impressive in their apparent originality—extremely strange and foreign to the imager's personality. We should certainly wish to endorse Leaning's contention that the imager's own beliefs greatly affect his interpretation of his experiences. The following hypnagogic image lends itself to supernatural interpretation, granted appropriate beliefs. 'I see a figure draped in white approaching my bed. It is vague but it is the figure of a woman and I get the impression she is a nun.... The figure I see sometimes speaks, or rather seems to be saying my name.' (Male, 18 years 10 months.) Two of the subjects in group B were a mother and a daughter, both strongly hypnagogic. The mother interpreted her images supernaturally, the daughter in naturalistic terms; both were very decided in their opposing interpretations.

What is true about the influence of antecedent beliefs on the interpretation of hypnagogic images also applies to psychotic beliefs. A psychotic might readily interpret a hypnagogic image in terms of his delusions, and if, as seems highly probable, hypnagogic images have enriched the folklore of mankind, they might similarly enrich the content of a pre-existing psychosis. A curious paradox arises, that to understand abnormal mental occurrences more fully, it may be necessary to examine abnormalities themselves in terms of *seemingly* abnormal experiences which a psychotic merely shares with many non-psychotic normal individuals. Consider the image reported by a student: 'I see skeleton figures, firstly very thin, then very fat. They appear alternately. The thin ones usually appear white and the fat ones black. I see this very often.' (Female, 20 years 2 months.) This is only one of the numerous frightening hypnagogic images reported, and a psychotic might also be frightened by such a psychotic-like experience, even although it is shared by normal people.

The fact that hypnagogic images can provide an individual with experiences greatly different from those he might otherwise have may be noted. One category of hypnagogic images involved the experience of change of size of the subject's own body or of things seen. One subject reported seeing people known to him who 'always grow bigger and bigger until I cannot picture them, or fade away to a single point'. (Female, 20 years

2 months.) Another subject reports a kinaesthetic image: 'all my body was swelling like a balloon and was being held together by thin bands of string in place of bones'. (Male, 22 years 11 months.) He states that the experience often preceded a standard nightmare. These images permit an individual to undergo experiences like those in a fanciful children's story—for example, two well-known incidents in *Alice in Wonderland*. They are also similar to the sensory experiences that form the basis of psychotic delusions of bodily change. Similar experiences occur to investigators who have experimentally induced psychotic-like states in themselves with such substances as mescaline and lysergic acid. The first writer after being given 0.5 gram of mescaline experienced a distinct sensation of elongation of one side of his body, this incidentally being reflected in the human figure drawings he produced at the time. Again, one subject who had taken an unstated dose of lysergic acid reported: 'As I watched, my fingers grew into my hand, my hand telescoped into my arm...my legs were now shrinking...my head, which was now double its normal size...' (Katz, 1953.) The possibility that a size-distorting mechanism, not unlike that occurring in psychosis and in experimentally induced psychotic-like states, operates for some subjects during the process of falling asleep merits further investigation.

It is possible to divide visual hypnagogic images into two classes. For want of better terms we shall refer to these respectively as 'perseverative' and 'impersonal'. The perseverative images are those easily located in past experience. A typical instance is the hypnagogic image of 'sometimes a clear picture of someone I had perhaps met during the day but I am not thinking about at the time'. (Female, 18 years 9 months.) The impersonal images differ; they are not easily explained in terms of past experiences, and may seem interestingly or alarmingly foreign to the personality. One subject reports 'a large bloated yellow head, pouting red lips, wild pale blue eyes rolling, hair dishevelled... The mouth was moving but there was no sound'. (Male, 21 years 8 months.) Another subject reported a similarly unexplained and similarly frightening series of faces experienced with open eyes. Some were impressed with the images as artistic subject-matter. Thus one subject reports visual images 'of a scene which I feel I would like to paint—these scenes are usually original'. (Female, 22 years 11 months.) These impersonal images, at times almost psychotic-like in their content, present difficulties of explanation that do not arise with the perseverative type of image. It is, nevertheless, our contention that these hypnagogic images, like other psychological occurrences, are subject to the principle stated by Hobbes: 'There is no conception in a man's mind which hath not at first, totally or by parts, been begotten upon the organs of sense.' (*Leviathan*, pt. 1, ch. 1.) With some success in a number of instances we have sought to defend this principle against 'archetypal', supernatural or other unsatisfactory hypotheses, by studying impersonal images with free association. An illustration may be taken. The subject, who was himself a clinical psychologist (age 31 years 3 months), reported in interview the following hypnagogic image: 'A beautifully formed and perhaps varnished claw, and only a claw. I suspect there was a foot originally and that it turned to a claw. Vividly and accurately coloured—a much more accurate claw than I could draw, unless I could use oils. Background appropriate but couldn't describe. The claw was outstanding. A cloud effect the base of the picture—like backs of sheep—nothing definite—focus of attention on the claw.' This description was taken down verbatim without interruption, and the subject was unable to locate the content of the image in past experience. A period of free

association with occasional questions followed. Space prevents giving the associations in detail but they included; 'the claw has dog's hair... the dog lying on the mat in front of fireplace with extended claws... base of picture could have been bank of cloud seen from aircraft'. The subject reported having done quite a lot of flying recently. He was now able to recall that a few days before he had seen a stuffed animal's head with highly polished horns. The mounting of the claw was similar to the mounting of this head. He also recalled a surrealist painting with something in it corresponding to the position of the claw in the image. Eventually the subject related a personal problem which had been causing considerable irritation. With his wife and two children, he had been staying with a relative; he had been repeatedly resentful of the sentimental attitude of the relative to her dog, and the preferential treatment the dog had received in any conflict between it and the children. The inquiry by free association eventually resulted in the reduction of the hypnagogic image to a dramatization of the threat to the children which the dog represented. The dramatization was built up out of past experiences. While this instance, like others also studied by free association, does not prove the hypothesis that images derive ultimately from sense perception, it at least illustrates the possibility of its validity. It also indicates that when conscious recall is not able to provide explanation of either the motivation or the content of a hypnagogic image, subsequent free association may help to supply this omission. The possibility that the hypnagogic image is like the dream in being a valuable clue to the personality and its emotional preoccupations may prove an extremely fruitful point for future investigation.

V. NOTES TOWARDS A THEORY OF VISUAL HYPNAGOGIC IMAGES

It has been suggested that the content of the hypnagogic image is provided by perceptual experiences. It is further suggested that the explanation of hypnagogic images must take account of the sense organs and central nervous system of the experiencing subject. It is now established that the brain has a spontaneous electrical activity, and that sense impressions are occurrences *superimposed* upon this spontaneous, autonomous activity which goes on all the time. As well as the spontaneous activity of the brain there is the similar activity of the sense organ, in this instance the retina of the eye. Without any external stimulation this spontaneous activity may yield a changing sequence of shapes and hues. As William James puts it: 'The retina itself, even in complete objective darkness, seems to be always the seat of internal changes which give some luminous sensation.' (1892, p. 46.) In a most interesting passage Galton (1883) candidly admits his own inability for a very long time to recognize this fact: 'I should have emphatically declared that my field of view in the dark was essentially a uniform black... I have found this is by no means the case, but that a *kaleidoscopic change of patterns and forms is continually going on*, but they are too fugitive and elaborate for me to draw on with any approach to truth. I am astonished at their variety.' (P. 114, italics ours.) A very careful study was made by Ladd (1892) of these products of retinal activity to which Wundt had given the name of 'luminous dust'. (They are alternately called '*Eigenlicht*', 'idio-retinal sensations' and 'phosphene' experiences.) Ladd reported: 'By far the purest, most brilliant and most beautiful colours I have ever seen, and the most astonishing combinations of such colours, have appeared with closed eyes in a dark room.' (P. 300.) It was after careful introspective study of these spontaneous retinal processes in an attempt to verify

Müller's theory of their relation to sleep that Ladd came to accept the view that the forms and colours constituting 'luminous dust' provide the essential basis of dream images. The question arises whether some extension of this theory might not be applicable also to hypnagogic images of the visual kind. The unusual and unexpected forms which hypnagogic images can assume present a difficulty for any explanation wholly in terms of reorganizations of *past* perceptual experiences. Some explanation more compatible with established psychological and biological knowledge than eruptions of a 'collective unconscious' and its attendant 'archetypes' seems desirable. This alternative is perhaps to be found in the interaction of present sensory experiences of the 'luminous dust' kind with past perceptions and their reactivation. Of such luminous dust Ladd reports: 'I verily believe *there is no shape known to me by perception or by fantasy*, whether of things on earth or above the earth or in the waters, *that has not been schematically represented by the changing retinal images* under the influence of intra-organic stimulation.' (P. 300, *italics ours.*)

Four characteristics of the visual hypnagogic images we have encountered in our investigation seem to require special explanation. There is, first, their frequent reported vividness; secondly, their autonomous character and independence of conscious control; thirdly, the original (unreproductive) character which at least some of them possess; and, fourthly, their ever-moving, or ever-changing, nature. If contemporary sensory processes play a part in the production of hypnagogic images (and, perhaps, as Müller and Ladd have suggested, of dream images), this would help to explain their vividness. The immense variety and rapid change in luminous dust to which Ladd refers—occurrences that are wholly outside voluntary control—would help to explain the autonomous and seemingly 'archetypal' character of visual hypnagogic imagery. It is not necessary to rest any such explanation wholly upon spontaneous retinal processes as opposed to those of a stimulus induced kind. Inadequate stimuli such as pressure, as can be readily verified, will greatly augment these retinal sensations. As Müller pointed out, when presenting an earlier version of the theory of dream images which Ladd developed, during sleep 'both eyes are turned inwards and upwards' (1848, p. 1413). The adjustment of the eyes in going to sleep would be likely to produce a considerable amount of inadequate stimulation of a kinaesthetic kind. Nor can we exclude the possibility that other physiological changes, attendant on the process of falling asleep, may also provide inadequate stimuli, though of an internal kind, either to the retina or to the visual areas of the brain. Müller points out (1848, p. 1343) that inflammation of the optic nerve will produce 'luminous phantasms'. Presumably, biochemical stimulation gives rise to the visions produced by such drugs as mescaline and lysergic acid. The reader is reminded of the similarity noted between mescaline-induced visions and the first writer's own hypnagogic images.

The kind of relation envisaged between luminous dust (with these possible augmenting sources of inadequate stimulation) and hypnagogic images may be indicated. Hypnagogic imagery, we suggest, does not occur against a screen of darkness, but against a changing screen of patterns and colours on to which the central processes are 'projected' (the term is used in an analogous sense); and this 'screen', being a non-homogeneous field of changing patterns, contributes to the resulting image. The outline of the images is provided by the luminous dust, but these are interpreted on the basis of such past perceptions as are reactivated. The situation indicated is more like highly personal interpretations being given to unstructured sense data (as in a projective test) than to simple free

associative activity in the absence of external determinants. Given such an interaction of central processes (past perceptions) with sensory processes of the retinal-dust kind (present perceptions), it seems possible to explain both the unexpected and impersonal character of such imagery, with the possibility that it may nevertheless provide information about the subject's personality, as do dreams. It is widely accepted that both exteroceptive and interoceptive sensations, as well as associative processes, play some part in the dream. That contemporary sensory processes, namely retinal dust, contribute substantially to the explanation of visual hypnagogic imagery seems a reasonable hypothesis for further investigation.

Visual hypnagogic images are sometimes, though rarely, found to occur with open eyes. Luminous dust may also be relevant to such open-eyed experiences. It has been noted that Galton postulated a continuum from ordinary imaging to hallucination. A little investigation showed the writers that retinal dust, continuous with that experienced with closed eyes, can be detected in twilight conditions with open eyes. The fact that apparitions are perceived or misperceived more readily in twilight than in broad daylight may be noted. It is possible to proceed further, and careful introspection reveals that luminous dust of an achromatic kind—a sort of mottled atmospheric haze—can be perceived even in broad daylight. This is difficult to observe, since the perceptual equivalents of spontaneous retinal activity are ordinarily suppressed by daylight. In Gestalt terms, they form an extremely insignificant 'ground' to the strong 'figure' which perceived objects present. Retinal dust is so omnipresent as to be readily overlooked, and was overlooked (even with closed eyes) for a long time by as acute an introspectionist as Galton. William James formulates a principle that may help to summarize our argument: 'Any objective stimulus to be perceived must be strong enough to give a sensible increment of sensation over and above the idio-retinal light.' (1892, p. 46.)

To this principle we would add that, when such increment is wholly or mostly absent (in darkness or with closed eyes), idio-retinal light is conspicuous, while in daylight it is barely discernible. We have suggested the probable relevance of such idio-retinal light to hypnagogic images, and Ladd has suggested its relevance to dream images. The possibility remains that, even in the presence of stimuli that give sensible increment, it may exert an effect. What is ordinarily ground may become figure, and in semi-darkness may give rise to apparitions and other such experiences interpreted supernaturally. There remains the possibility of its relevance to hallucinations in full daylight. Cameron (1947, p. 542) reports the tendency of a delirious patient to experience 'dream-like hypnagogic hallucination when he shuts his eyes' before passing to actual hallucination with open eyes. Galton suggests that some very slight physical change 'short of really morbid character' would enhance the vividness of luminous dust experiences. This suggestion is interesting, since in the typical kinds of psychosis in which visual hallucination is a prominent symptom there are known to be biochemical or other physiological processes at work; these might well supply inadequate stimulation sufficient to induce the postulated figure-ground reversal.

VI. SUMMARY

An investigation of hypnagogic images was conducted on three groups of Aberdeen University students and staff. Provisional quantitative data are given, but the main concern of the investigation has been to collect descriptive accounts of hypnagogic

images, to classify these images, and to outline a theoretical explanation of those of a visual kind. It is suggested that hypnagogic imagery may throw light upon a number of wider issues including some forms of creative thinking, and various paranormal phenomena, notably 'visions'. Data suggest that hypnagogic experiences may help to fill in gaps in the continuum which Galton supposed to extend from complete inability to image to gross hallucination. Hypnagogic images of an auditory type—the commonest kind in the group studied—are not readily distinguishable from hallucinations; visual hypnagogic images both resemble and differ from dreaming and ordinary waking images. The theory that the spontaneous activity of the retina plays an important part in visual hypnagogic imagery—a development of Ladd's theory of the dream—is considered and defended as helping to account for a number of characteristic features of the images.

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Note. An historical treatment of hypnagogic imagery from the time of Aristotle is to be found in Leaning (1925), who also gives a comprehensive bibliography.

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SOME EFFECTS OF INCENTIVES ON THE PERFORMANCE OF IMBECILES¹

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I. INTRODUCTION

Although a few investigators have carried out experiments with imbeciles in which incentives have been used (Aldrich & Doll, 1931; Gardner, 1945) the problem of how low-grade defectives respond to incentives remains essentially unexplored. It has been suggested that imbeciles constitute a separate clinical entity and that certain basic operants of behaviour found among normals or even feeble-minded persons do not apply to them. Among others, Lewis (1948), and Kallman (1951), maintain that while high-grade defectives form the lower end of the normal distribution curve of human intelligence, low-grade defectives do not fit into any scheme of continuous variation. Whether qualitative differences in intelligence affect all aspects of behaviour is, however, not known.

With the general aim of contributing towards a more systematic psychology of the imbecile defective this investigation seeks to describe how imbeciles² respond to different incentives, and to determine to what extent their behaviour in a test situation is modifiable by experience or incentive.

In order not to have to cope with the difficult problem of complex variables involved in previous training, it was decided to select a task which was essentially unrelated to prior experience and which could be measured precisely. A measure of persistence, operationally defined as the length of time an individual maintains an uncomfortable or tiresome position, seemed suited to our purpose and the Leg Persistence test was chosen to meet these requirements. The test requires the subject to sit on a chair and hold one of his legs horizontally above another chair, keeping the heel of his shoe about an inch from the seat of the second chair for as long as he can. The choice of the Leg Persistence test was only incidentally related to investigations on persistence as such.

II. SOME ASSUMPTIONS

We assumed that the Leg Persistence test was of no intrinsic interest to the subject; that is, the subject would not enjoy doing the task itself. He would probably be willing to comply with instructions either to please the examiner or, perhaps, to avoid getting into

¹ The present study is part of research in social and psychological aspects of mental deficiency carried out under the direction of Prof. Aubrey Lewis. Some of the work reported in this paper was presented for the Ph.D. degree at London University by S. Gordon.

² The sample used in the investigation reported here includes only persons about whom there is little doubt regarding their classification as imbeciles, whether one uses the criteria employed by Lewis (1929), Doll (1946), or Tredgold (1947).

trouble if he believed that the examiner was a person in authority. Over and above motivations such as these, it was expected that imbeciles—if they responded to incentives in a manner resembling normals (as described for example by Mace, 1935)—would modify their performance of this task according to the following pattern. Those working towards an adjusted and ascertainable goal would perform significantly better (i.e. persist longer) than those who were constantly encouraged to do their best; and both would be significantly superior to a control group. However, it was problematical whether imbeciles would be able to comprehend the significance of a goal as such and it was thought therefore that they might respond better to encouragement than to knowledge of results or self-competition.

III. SELECTION AND DESCRIPTION OF THE SAMPLE

The subjects were twenty-seven males certified as imbeciles under the English Mental Deficiency Laws and resident patients in a large hospital for mental defectives.

They were selected in the following manner. The case histories of patients in the two wards of the hospital which contained the largest number of male imbeciles were examined—some sixty in all. All those not doubly handicapped, such as the blind, severely crippled or epileptic, and who were between 17 and 35 years of age were selected to participate in the experiment. No attempt was made to eliminate those said to be emotionally unstable or psychotic. The majority of those not included in the experiment were patients above the age of 35. In all, thirty-one patients were individually given the Leg Persistence test. Four patients (of five diagnosed as psychotic) either did not understand the instructions or refused to co-operate and were not included in the experiment.

The age,¹ data on the number of years spent by these patients in institutions for mental defectives, and their Terman Merrill I.Q.'s, are summarized in Table 1. The subdivision

Table 1. *Subjects who took part in Leg Persistence experiments*

Means, range, and standard deviations for Age, number of years spent in institutions and Terman-Merrill I.Q. of each group of subjects, and for all subjects.

Group	Age		
	Mean	Range	S.D.
Control	28.78	21-35	4.30
Encouragement	26.11	23-32	2.86
Goal	25.78	21-31	3.14
Total	26.89	21-35	3.74
Group	Years in institution		
	Mean	Range	S.D.
Control	16.67	10-22	3.83
Encouragement	18.89	7-24	4.86
Goal	19.0	14-24	3.78
Total	18.19	7-24	4.38
Group	I.Q.		
	Mean	Range	S.D.
Control	33.33	25-42	6.61
Encouragement	34.88	26-41	5.06
Goal	37.66	31-48	4.88
Total	35.29	25-48	5.86

into three groups is explained in section IV (p. 279). The mean age was 26.89 years, S.D. = 3.74 years. All but a few had spent the greater part of their lives in an institutional environment. The 1937 Stanford Revision of the Binet test (Form L) was given to each

¹ Given to the nearest whole year as at the beginning of this experiment in October 1952.

subject after the entire investigation had been completed. The results were in keeping with the diagnosis of imbecility in all cases. The mean I.Q. was found to be 35.29, s.d. = 5.86, with a range of 25-48.

Most of the subjects were described in their case notes as being 'capable of only limited work under constant supervision'. Apart from the classification of imbecility, only a few of them appeared to be of well defined clinical types. Two were mongols, and four or five were almost certainly brain injured. Three were believed to have had at least one defective sibling but as far as could be determined none had parents who were defectives. One patient was described as schizophrenic and four others as emotionally unstable. The case notes indicated that developmental retardation was apparent in all cases and had been noted by the parents or guardians before the fourth year and in most cases by the third year.

None of the subjects was able to answer correctly the questions, 'How many do 3 and 4 make?' and, 'If you had three sweets and four sweets, how many would you have altogether?'

IV. LEG PERSISTENCE: EXPERIMENT ONE

(i) *Experimental design*

Each subject was individually given the Leg Persistence test. The procedure was demonstrated by the examiner before the first trial. The subject was seated with a chair opposite him and the following instructions were given: 'When I give you the signal I want you to hold up your leg above this chair as long as you can without touching the chair. Remember, hold it up as long as you can without touching. Ready? Go!' No further comment was made until the leg touched the chair. The examiner then said, 'Good work, I'll see you again tomorrow'.

The score recorded was the number of seconds the subject held his leg in the air. A second trial with the same instructions was given the following day. Although the subjects were not given any instructions as to which leg to use, it was noted that all subjects used the same leg each time.

The higher score of the two trials was employed as a basis for dividing the twenty-seven subjects into three matched groups of nine persons each. (No other variable was used in the matching.) The subjects were ranked from best to worst according to their score on the test. The three subjects with the highest scores were grouped and each one was assigned at random to one of the experimental conditions given below. Similarly the following three were grouped and the same procedure of assigning each one at random to one of the experimental conditions was repeated, until the twenty-seven subjects were assigned to one of three experimental groups.

The Leg Persistence test was given individually as before to members of the control and two experimental groups once a day for ten consecutive days, excepting a week-end break after the fifth day. The groups were differentiated in the following manner:

Control

The original instructions. No change.

Encouragement.

In addition to the original instruction some remark of encouragement was made by the examiner every 5 sec. By the third trial this followed a quite stereotyped pattern;

'Keep it up... Keep it up as long as you can... That's good... You are doing fine... You want to try to keep it up as long as you can...'

Goal (sometimes referred to as knowledge of results).

The subject was shown a piece of cardboard (8 × 20 in.) on which was fixed a movable pointer. This could be manipulated by the examiner. A scale running from zero to 300, marked on the side of the cardboard, like the markings on the side of a mercury barometer, was used to indicate the number of seconds that elapsed. For the first experimental trial a paper clip was placed at a point 10% higher than the subject's best previous performance (original matching score). On the second and subsequent trials the clip was placed at a point 10 sec. above their best previous performance. The examiner pointed to the clip and said: 'This is as far as you got the last time. See if you can hold your leg up until this pointer passes the clip. Let us see if you can beat your record. Ready? Go!' The pointer was moved up from the zero point by a string pulley at 5 sec. intervals according to the marked gradations. The subject was told that no talking was permitted during the experimental period and the examiner himself made no further comments. At the end of the session if the subject reached his goal he was told, 'You reached your goal today. Good show. We will see if you can reach it again tomorrow'; and if he did not reach the goal, 'You did not reach your goal today. You will try again tomorrow'. On the third trial and for each subsequent trial the procedure was again slightly modified. In order to avoid frequent failures, success was contrived if the subject was within 25 sec. of his goal. This was done by pulling the pointer up to the clip just as the subject seemed ready to put his leg down.

(ii) *Experimental conditions*

A private room adjoining the wards was available for the experiment. Each subject was tested shortly after breakfast or after dinner. There were no obvious distracting influences during the experimental period. Discussion of any kind during the experiment was discouraged and all questions, of which there were many, were answered evasively. To a variety of questions such as, 'What is this for?' the reply was, 'You will see later on'.

The examiner was very well received by the patients. Rapport was good in all cases. The Charge Nurse reported that most of the subjects looked forward to the coming of the examiner each day.

(iii) *Matching of groups*

The data, as summarized for each group, for the unmatched variables of age, years spent in institutions, and I.Q. are presented in Table 1. The three groups are well matched on these uncontrolled variables, and none of the differences between groups were significant.

An analysis of variance¹ was carried out to determine whether there were any significant differences between the three groups in their initial scores on the test (the controlled variable). The means and standard deviations were respectively (Goal, $\bar{X}$ = 92.56; s.d. = 27.42; Encouragement, $\bar{X}$ = 87.89 s.d. 40.40; Control $\bar{X}$ = 89.89 s.d. = 41.34.

For 2 and 16 degrees of freedom the *F* ratio was 0.306 and the null hypothesis was therefore accepted. There were very significant within-group differences, the mean score for the highest performer in each group being 163 sec., while the mean for the lowest

¹ All analyses of variance are for matched groups and follow the procedure given in Edwards (1950), pp. 274-6.

performer was 37 sec. There was a low but positive correlation ($r=0.355$ between initial scores on the Leg Persistence test and I.Q.

Trials 1-10

(iv) *Results*

The introduction of incentives effected striking differences between the three groups which began at virtually the same level. (The results, as indicated by the actual mean scores for each trial are presented graphically in Fig. 1, according to groups.) The Control

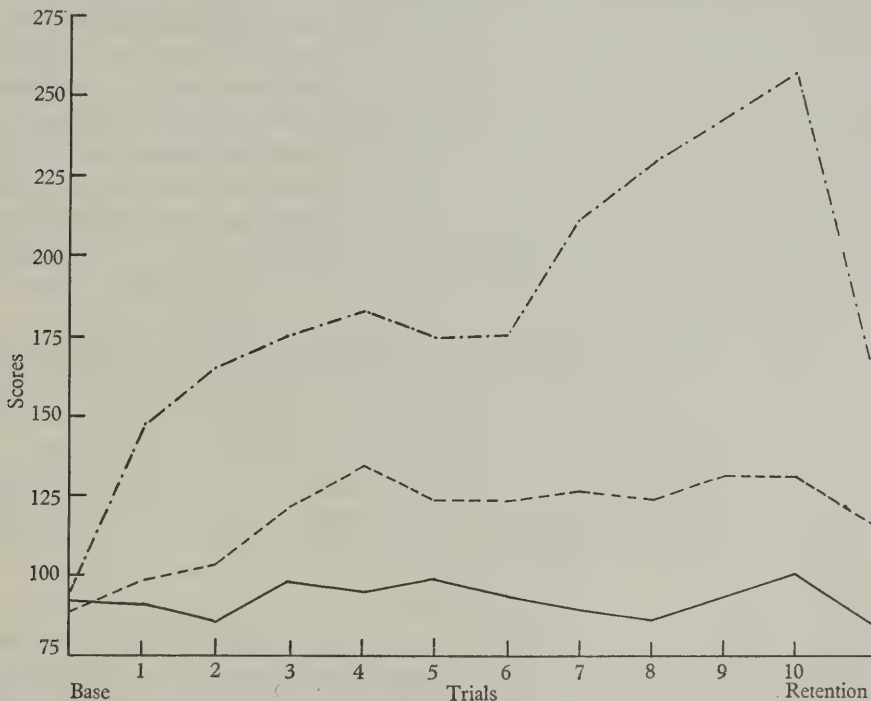


Fig. 1. *Leg Persistence*: mean score base, trials 1-10, and retention for Control, Encouragement and Goal groups. —, Control; ---, Encouragement; - · - · -, Goal.

group averaged from 85 to 101 sec. daily, the Encouragement group from 89 to 134 sec., and the Goal group from 146 to 257 sec. An analysis of variance computed for the mean score of each subject (trials 1-10) in the original matching order, as well as for the mean scores of each group for each of the ten trials, showed that the differences between the groups were very significant at less than the 0.001 level of confidence when the mean score of each subject was used ($F=9.895$ for 2 and 16 D.F.). (Between Goal and Encouragement, $t=5.465$, significant at the 0.001 level and between Encouragement and Control, $t=2.415$, significant at 0.05 level of confidence.)

As can be seen in Fig. 1, the Control group made virtually no progress throughout and could almost be represented by a horizontal line. After moderate gains for the first four trials the Encouragement group declined slightly and then maintained a fairly constant level of at least 25 sec. above Control for each trial. The Goal group showed on the first trial a very sharp acceleration to a point which exceeded the highest score obtained by

its nearest rival—the Encouragement group. On the tenth trial the Goal group obtained a mean score which represented a gain of 123 sec. or 91% over the highest mean score obtained by the Encouragement group; and a gain of 156 sec. or 153% over the highest mean score in the Control group.

Other measures illustrate further the pattern of results obtained. The mean percentage of improvement represented by the average score for the ten trials compared with the original matching scores was 2.22 for the Control, 46.44 for the Encouragement, and 115.0 for the Goal group. When the highest single score obtained by each subject was used to compute the percentage of improvement, the mean percentage of improvement for each group was as follows: Control 35.10, Encouragement 85.22, and Goal 190.33.

No subject in the Goal group failed to improve less than 54% (mean score trials 1–10 compared with matching scores) and the greatest improvement was 209% achieved by a subject who was originally in the last matching. In the Control group the highest percentage improvement was 24% and three subjects obtained mean scores for trials 1–10 which were lower than their original matching scores. The range of percentage of improvement for the Encouragement was 0–98. The percentage improvement appeared largely unrelated to the tested intelligence. The correlation between I.Q. and per cent improvement was $r=0.240$ in the Control group, 0.099 in the Encouragement group, and 0.138 in the Goal group.

The rank order of subjects in the Control group remained almost the same throughout the experimental period. The correlation between the original matching score and the mean score for the first five trials was $r=0.97$ and for the second five trials $r=0.97$. The matching scores thus appeared to be highly reliable. When incentive conditions were changed the original rankings were also somewhat changed. The correlation between the base and the mean scores for the first and second weeks in that order were as follows: Encouragement $r=0.87$ and 0.80; Goal $r=0.79$ and 0.82.

(v) *Conclusions*

The results suggest that imbecile subjects, engaged in a tedious task of short duration, are capable of responding to incentives in an orderly and predictable way. Those striving to reach a goal related to their standards of achievement will perform consistently and significantly better than those who are simply encouraged, however persistently, to do their best, and both of these groups will show superior results to a control group. In their manner of response, imbeciles, it would seem, do not differ from normals (as suggested by some of the experiments of Mace). This conclusion must be tempered by the consideration that although the matching on the basis of performance appeared to be sound (as substantiated statistically) there is always a possibility, especially in a small sample, that some unknown variables have been operating to affect performance scores. Consequently it seemed desirable to carry out further experiments designed partly to check the original findings and partly to elaborate and refine our conclusions.

V. LEG PERSISTENCE: EXPERIMENT TWO

(i) *Introduction*

An experiment in which some members of each group were shifted to different conditions seemed a feasible way of examining more critically our previous results. If, for example, subjects from the Control group who changed over to Goal conditions improved their

performance, it would lead us to reject the possibility that the original results could be accounted for by errors of sampling. The experiment would also provide a basis for examining imbecile behaviour as affected by new incentive conditions.

(ii) *Experimental design*

The subjects, in each group separately, were ranked according to their average performance in the ten trials of the first experiment. Persons ranking 1, 3, 5, 7, 9 in the Goal and Control groups were selected to remain under the same conditions. The remaining subjects (ranks 2, 4, 6, 8) in the Control group were shifted to Goal conditions and those remaining in Goal were shifted to Encouragement. In the Encouragement group ranks 1, 3, 5, 7, 9 were shifted to Goal conditions and the even ranks remained as before. The experiment was conducted in the same manner as that described in the first experiment together with the changes as outlined; it was commenced on the Monday following the end of the 10th trial of the first experiment. It should be recalled that there were now six groups, based on the original Control, Encouragement, and Goal groups.

It will sometimes be convenient to make use of the following symbols:

- 5-C Five subjects under Control conditions for twenty trials.
- 4-CG Four subjects: ten trials Control, ten trials Goal.
- 5-EG Five subjects: ten trials Encouragement, ten trials Goal.
- 4-E Four subjects: twenty trials Encouragement.
- 5-G Five subjects: twenty trials Goal.
- 4-GE Four subjects: ten trials Goal, ten trials Encouragement.

(iii) *Results*

As we are now dealing with six new groups, we shall treat the data in terms of the full twenty trials. The results, employing the mean scores for each of the four 5-day periods for each group (the twenty trials were given from Mondays to Fridays for four consecutive weeks) are presented graphically in Fig. 2. (The smoothing procedure employed in plotting the graph did not do violence to the essential pattern of the results.)

As can be seen from Fig. 2 (ignoring for the present the retention score), several fairly clear patterns emerged. Of the two Control groups, 5-C remained more or less unchanged during the 4-weekly periods given, but 4-CG improved considerably when shifted to Goal in the third week, and also exceeded by the fourth week the performance of group 4-E, which remained under Encouragement conditions for twenty trials. The Encouragement group shifted to Goal (5-EG) obtained a higher mean score than 5-C, 4-CG, and 4-E, but remained well below both Goal groups. Both Goal (5-G) and Goal shifted to Encouragement (4-GE) groups remained almost identical throughout except for a slight drop in the scores of those in 4-GE when the shift was made. Ranking the groups according to performance scores for trials 11-20 we had the following order from best to worst:

- (1) Goal (5-G).
- (2) Goal shifted to Encouragement (4-GE).
- (3) Encouragement shifted to Goal (5-EG).
- (4) Control shifted to Goal (4-CG).
- (5) Encouragement (4-E).
- (6) Control (5-C).

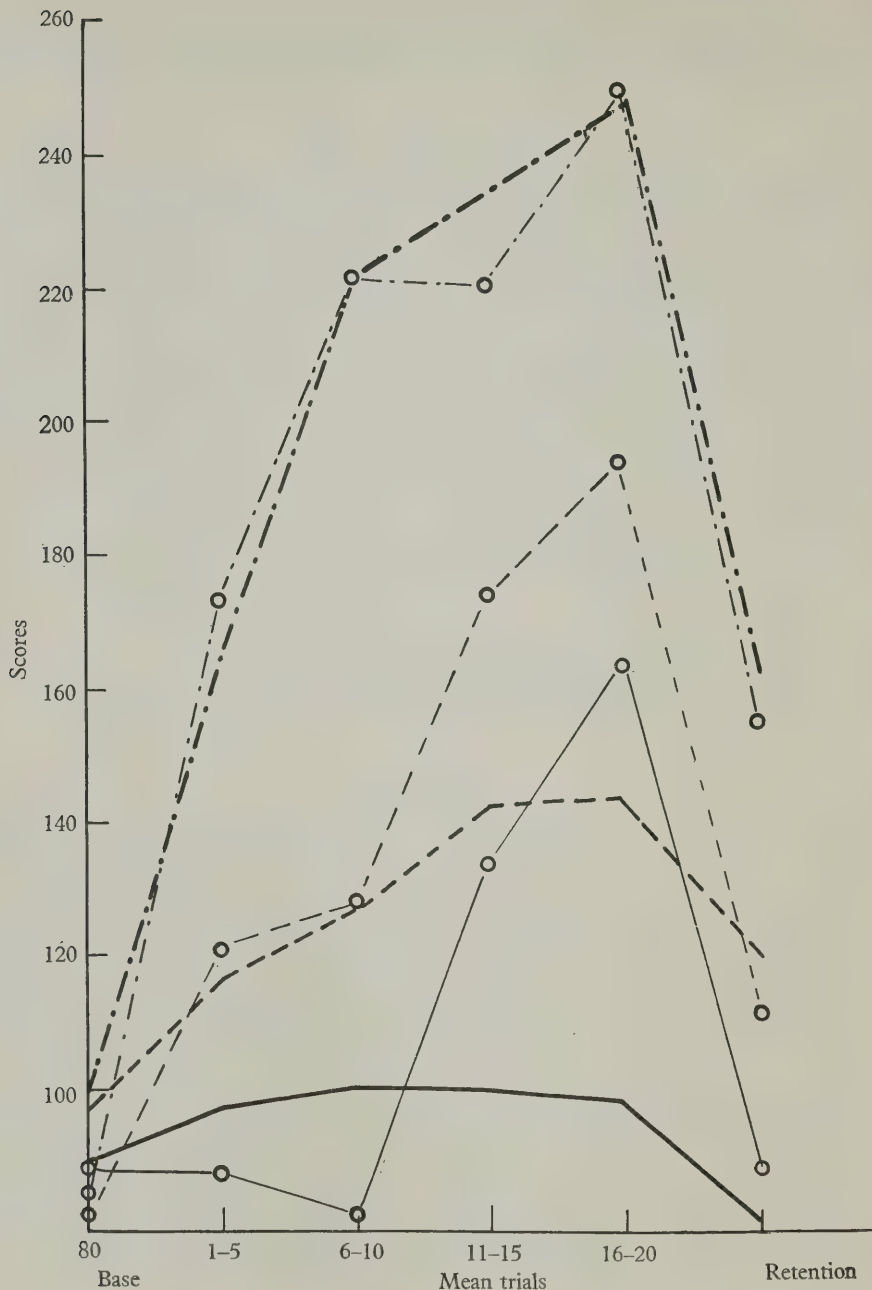


Fig. 2. *Leg Persistence*: mean weekly scores for shifted and non-shifted groups. 5-C, —; 4-CG, ○—○; 4-E, ---; 5-EG, ○---○; 5-G, -·-·-; 4-GE, ○-·-·-○.

To summarize, the results suggested that both (a) type of incentive, and (b) prior experience influenced the magnitude of the response. Thus an initially superior incentive showed its effect in superior performance even when replaced by an incentive known to be less effective. (Whether this effect would wear off after a time remained a question for

future experimentation.) If, however, a less effective incentive was followed by a superior incentive, the improvement in performance was not as great as that found when subjects began the task under superior conditions of motivation.

(iv) *Conclusions*

The data strengthened the view that the previously obtained results (Experiment One) were not chance findings and that imbecile subjects respond in an orderly manner to incentive conditions. With imbecile subjects, it appeared that *initial* levels of performance were important determinants of subsequent performance. Improvement in performance appeared to be a function not only of the incentives employed but of their sequence.

VI. LEG PERSISTENCE: EXPERIMENT THREE

(i) *Introduction*

To provide further experimental data on the effect of the incentives on subsequent performance the test was again given twice to each subject *under the original matching conditions* after an 8-week interval without practice. The scores obtained are referred to here as 'Retention Scores' by analogy with the retention scores obtained in memory experiments.

(ii) *Method*

After an 8-week interval the subjects were again given the Leg Persistence test under the original Control conditions—that is, a subject was told, when seated, to hold his leg above a chair placed opposite him as long as he could. The subjects were given two trials on subsequent days; the higher of the two scores constituted the measure of retention. During the 8-week interval all but three subjects participated in another series of experiments involving incentives but on an entirely different task.

(iii) *Results*

The retention scores for the six groups are presented graphically in Fig. 2. The results can be summarized in terms of mean scores for each group. They are presented in Table 2 and compared with the mean scores for trials 1–10 of the first experiment and trials 11–20 of the second experiment.

Table 2

Group	Mean score trials 1–10	Mean score trials 11–20	Retention
5-G	193·6	241·3	162·4
4-GE	197·3	234·9	154·3
4-E	121·3	143·3	120·3
5-EG	124·6	184·6	112·0
5-C	99·2	97·7	87·8
4-CG	85·4	148·6	81·0

On examination of the retention scores a curious result was observed. There were large differences between the best group (5-G) and the worst (4-CG). The rank order of the groups did not, however, correspond closely to their scores during trials 11–20. Instead it was the incentives under which they worked during trials 1–10 which seemed to determine the magnitude of their retention scores. If we combine the groups according to the original division into Control, Encouragement and Goal groups the retention scores

obtained by each correspond to the order in which the three original groups responded. This is shown in Table 3.

Table 3. *Scores on trials 1-10 and on retention for Control, Encouragement and Goal groups*

Group	Mean score trials 1-10	Retention ¹
Goal	195.5	158.4
Encouragement	123.0	116.2
Control	92.3	84.4

Table 4

Group	Retention (1)	Retention (2)
5-G	152.0	147.0
4-GE	144.3	127.5
4-E	120.3	91.5
5-EG	107.4	97.0
5-C	78.0	60.2
4-CG	86.3	64.5

The combined scores are also presented graphically in Fig. 1 (p. 281). The groups which were most alike during the first ten trials remained most alike after an interval of 8 weeks, despite the fact that during trials 11-20 the Control and Encouragement subjects shifted to Goal conditions improved considerably above the level of their counterparts, i.e. those who remained on Control and Encouragement conditions for the full twenty trials.

In order to examine the possibility that these results might be artefacts of the measure used (the higher of the two retention scores), it seemed profitable to examine the two scores separately. The results are summarized in mean scores in Table 4.

The general pattern of the results for either of the retention scores was the same as when the higher of the two scores was used. All mean scores declined on the second retention trial and twenty of the twenty-seven subjects obtained their highest score on the first retention trial.

(iv) *Discussion*

The results again seemed to emphasize the significance of initial achievement levels. Just why the Encouragement and Control groups shifted to Goal conditions (groups 5-EG and 4-CG) did not obtain retention scores which were superior to their non-shifted counterparts we do not know. Examination of the extensive, but by no means exhaustive, reviews of the literature on retention did not reveal an analogous experiment which could have served as a basis for comparison.

VII. CONCLUSIONS

Within the limits imposed by the experimental task employed to reveal some effects of incentives on the behaviour of imbecile subjects, the results led us to accept the view that imbeciles were capable of responding to incentives. Furthermore, as suggested by the first experiment, our subjects responded differentially to incentive conditions such as working towards a goal and encouragement in a manner similar to that noted amongst normal subjects in other investigations.

¹ An analysis of variance computed on the basis of the combined groups of the original matching and the retention scores gave an *F* ratio for the between groups variance of 5.102 significant at less than 0.05 level of confidence for 2 and 16 degrees of freedom.

The persistence test used appeared well suited to reveal these aspects of behaviour. It presented no special problems relating to factors such as prior experience, and scores obtained were largely unrelated to tested intelligence. The results could be objectively scored and were shown to be reliable. As it was a tedious task, devoid of any intrinsic interest, improvement was probably more a function of extrinsic motivation alone than would be the case when subjects were doing something they enjoyed. None of our subjects reported liking the task, but it seemed quite clear that marked improvement in performance was a result of very strenuous effort. The effort, as manifested in performance, was shown to bear a direct relationship to the nature of the incentive offered.

The results of Experiments 2 and 3 strongly support the view that, with imbecile subjects at least, initial levels of achievement are critical determinants of subsequent performance. Both the kind of incentive and the sequence in which it was given appeared to be more significant as factors in improvement than the actual number of trials. However, because of the very small numbers involved in the groups of the later experiments, and the lack of prior experimental work in this area which could serve as a basis for comparison, conclusions must be considered tentative and further discussion must await confirmation of the results.

What emerged most strikingly was that the imbeciles of this sample were 'good' subjects. The results of this exploratory study (involving 648 observations and unaffected by any absenteeism) followed an orderly and consistent pattern and permitted a certain amount of generalization. We gained what appeared to be a very useful basis for hypotheses and for planning further experimental work.

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CHILDREN'S REACTIONS TO BIVARIANT STIMULI

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I. *Introductory* (pp. 288–289). II. *Experimental* (pp. 289–291): (i) *Subjects and apparatus* (p. 289–290); (ii) *Procedure* (pp. 290–291); (iii) *Results* (p. 291). III. *Discussion* (pp. 291–293). IV. *Summary* (p. 293). *References* (pp. 293).

I. INTRODUCTORY

One of the major functions of a theory is the parsimonious summarizing of facts. This paper attempts to show that the facts of generalization behaviour as shown by children in the transposition situation may be regarded as special instances of the generalization which occurs in the wider bivariate stimulus situation. This latter situation arises when the stimuli, between which the child must discriminate, offer two differential stimulus components, e.g. there may be both a colour and a form difference as when one stimulus is red and round while the other is green and square. It is the situation made familiar by Descoedres (1914) and later workers in the investigation of the dominance of colour over number, of form over position, etc. It is basic to conditional and pattern discrimination and is generally representative of those complex situations which are variously described as setting multiple-sign, higher order generalization, or ambivalent-cue problems. A brief review of these situations is given by Harlow (1951).

Suppose a young child is repeatedly presented with two visual stimuli which differ in size and is trained to react always, say, to the smaller. If he is then faced with two new visual stimuli which also differ in size, the child may, under appropriate conditions, transpose, i.e. react to the smaller of the two test stimuli in the same way as he formerly reacted to the smaller member of the training pair. It seems fairly certain (Hunter, 1952) that the frequency of such transposition is an increasing function of: (1) increasing similarity of the two training stimuli to each other in their non-size stimulus characteristics and, within the limits of the discrimination threshold, in their size characteristics; (2) increasing similarity of all the stimulus characteristics of the test pair to those of the training pair; (3) within limits, increasing amounts of practice on the training pair; and (4) increasing mental age of the child.

The first fact above, referring as it does to the conditions favouring abstraction of relational stimulus characteristics, must enter explicitly into any theoretical formulation of transposition phenomena. But, implicit in the writer's previous discussion of the remaining facts (Hunter, 1952, 1953) is the suggestion that they might be explained in terms of two theoretical assumptions. The first assumption (of generalization-by-similarity) is that the amount of generalization shown to any test situation is a direct function of the similarity of that situation to the training situation. Similarity is thought of as occurring along stimulus dimensions which are constructed in terms of either the physical or response properties of the stimulus (or of response properties alone since, on analysis, the former properties reduce without remainder to the latter). The second assumption is that the transposition situation is a special case of the bivariate stimulus situation. It is supposed that, during training, the child learns to react simultaneously to two components of the visual situation which are, logically at least, separable. One component is absolute

and involves the specific stimulus characteristics of the entire training situation, including the training stimuli themselves; the other is the relation of 'larger than' or 'smaller than' which, being qualitative, is present or absent in all-or-none fashion.

According to those two assumptions, the frequency of transposition to a test pair which possesses the relational component (i.e. the members of which differ discriminably in size) will depend on the similarity of the absolute characteristics of that pair to those of the training pair. The test pair most similar to the training pair is, of course, the training pair itself which may be thought of as eliciting transposition with the greatest possible frequency. At the other extreme lies some test pair which, absolutely, is so dissimilar to the training pair that the child will fail to react consistently to it in any way whatever. The effects of practice and of mental age on transposition are explained simply by their permitting generalization to take place to test pairs whose absolute characteristics are increasingly dissimilar to those of the training pair. To illustrate, consider generalization at two different stages of training, one shortly after the discriminative reaction has been established and the other after overtraining. At the first stage, reaction to the test pair is haphazard, i.e. a change in the absolute component brings about discrimination breakdown. At the second stage, the same change in the absolute component does not produce breakdown and transposition appears.

The above way of restating the facts, while admissible, is hardly parsimonious unless some general statement can be made about the interaction between the two hypothesized stimulus components and the influence which practice and mental age have on that interaction. The experimental part of this paper is directed toward providing such a statement.

The validity of treating transposition in terms of reactions to a bivariate stimulus situation cannot, however, be checked directly within the transposition situation itself since the two stimulus components are not, there, independently manipulable. Specifically, while the absolute component can be altered without disturbing the relative, the latter cannot be removed without tampering with the former. In order to study each of the two components, a bivariate stimulus situation must be devised which meets two requirements. First, each component must be independently variable with regard to the other. Secondly, one component must not be so very much more readily reacted to than the other that only the dominant one enters effectively into the picture. The discrimination situation to be described below meets both requirements.

II. EXPERIMENTAL

(i) *Subjects and apparatus*

The subjects were twenty-seven nursery school children (twelve boys, fifteen girls) with an age range of 38–60 months and an average age of 50.2 months.

The training and test pairs each comprised two 6 × 6 inch stimulus cards presented, side by side, on an apparatus so that, immediately beneath each, was a small drawer. This simple presentation apparatus has already been described (Hunter, 1952). The child discriminated between the two cards by opening the drawer beneath one of them. During training, there was always a raisin in the drawer beneath the positive card and, during testing, there was a raisin in each drawer. The cards, cut from stiff white cardboard, were painted mat black to leave a white geometrical figure exposed in the centre. These figures

were bivariant being made up of varying sizes of upright equilateral triangles broken by alternating black-and-white horizontal stripes of varying width. There were three sizes of triangle—'large', 'medium', and 'small'—with sides 11.3, 9.5, and 8.0 cm. long, respectively. The area of the large triangle was, therefore, 1.4 times that of the medium which, in turn, was 1.4 times that of the small. Likewise, there were three widths of stripe—'thick', 'medium', and 'thin'—which measured 0.4, 0.3 and 0.2 cm., respectively. The black and white stripes were of the same width on every card so that any difference in the total brightness of two cards was due entirely to the different sizes of the triangles on them. Since linear size and total brightness co-varied and since their separate contributions to discrimination were not being analysed, these two characteristics were simply treated as constituting the single characteristic of 'size'.

Eight stimulus cards were prepared, each bearing one of the nine possible size-width combinations (the medium-medium combination was not required). These eight combinations were: large-thick, large-medium, large-thin, medium-thick, medium-thin, small-thick, small-medium, and small-thin. Because the altitudes of the triangles were not all exact multiples of the stripe widths, incomplete stripes at either apex or base might have furnished unwanted differential features. To avoid this, the altitudes were altered very slightly to make them exact multiples of the particular stripe width with apex and base stripes both white. These eight cards were split into two training pairs and two test pairs. One training pair was: small-thin (+) *v.* large-thick (-). The other training pair was: small-thick (+) *v.* large-thin (-). Both pairs provided the two components of differential size and differential width. The two test pairs, on the other hand, provided only one component each, the differential features of the other being removed. The test pair which provided only the differential size component was: large-medium *v.* small-medium. The other test pair, providing only the differential width component, was: medium-thick *v.* medium-thin.

(ii) *Procedure*

On being brought into the experimental room, the child was seated in front of the presentation apparatus, shown the cards and the drawers beneath them, and allowed to take a raisin from one of the drawers. Training was then begun. The training pair was presented and the child told, by word and gesture, to 'look at both of the cards', then to 'point to the one that has the raisin', and then to 'open it'. If the drawer beneath the positive card was opened, the child found a raisin which he could eat. If the 'wrong' drawer was opened, the child then found that he could not open the other and was told that he must open the 'right one' first otherwise he would not get the raisin. This was the first trial. For the second trial, the positions of the cards were changed and the above procedure repeated. Thereafter, the two training cards, their relative positions being changed in random order, were presented repeatedly until the child reached the arbitrary learning criterion of five consecutive trials correct. With the older children, it was not necessary to repeat the three separate commands on all trials; it sufficed to say 'Choose'. But unless all three directions were consistently given to the younger children, they began to react to the drawers without looking at the cards at all. Preliminary observations showed that it was the task of manipulating some part of the apparatus other than the stimulus cards themselves which made this discrimination arrangement too difficult for very young children. Fourteen children were trained with one training pair, and thirteen with the other. All children were tested with both test pairs.

When the child had reached the learning criterion, he was given three consecutive presentations of one or other of the test pairs. Training was then resumed until two consecutive training trials were correct, whereupon three trials were given with the remaining test pair. Thereafter, training trials were continued with groups of three test trials interspersed irregularly among them. The child was regarded as showing generalization to a particular test pair when he chose the same member of that pair on three consecutive trials regardless of position. The experiment was concluded when the child generalized to both test pairs. Only one subject refused to continue before reaching the end of the experiment.

(iii) *Results*

On the average, the children gained the learning criterion in seventeen trials and, after a further twenty-two training trials (this figure does not include test trials), showed bidimensional generalization, i.e. generalization to both the size and width components when each is presented in isolation from the other. With six subjects, a further twenty training trials in no way altered this bidimensional generalization, suggesting that it was a stable end-stage. (These averages do not take account of the one child who finished before the experiment was completed. He gained the learning criterion in seventeen trials and left the situation after a further six training trials at which time he was generalizing in terms of size but not of width. However, there was no reason to doubt that, like the other children, he would have generalized to both components had he continued. In presenting the results below, it will be assumed that he did continue and did show bidimensional generalization.) Once the child had reached the learning criterion, few if any mistakes were made on subsequent training trials. When, during testing, one card was not consistently chosen, the usual reaction was to position, e.g. always to the left side. Choices against the direction of training, in the sense of having been trained to choose the small triangle and then choosing the large triangle in a test trial, often appeared but never consistently.

In the test trials, four different types of generalization were possible, i.e. generalization to neither component (discrimination breakdown), generalization to both components (bidimensional generalization), generalization to the size component only, and generalization to the width component only. Thus, individual performances could differ from each other in terms of the number of types of generalization shown and in the order in which they made their appearance as training proceeded. In terms of such differences, the children fell into four groups. (1) Nine children (age range, 49–60 months; mean age, 57 months) showed only bidimensional generalization. (2) Seven children (age range, 39–53 months; mean age, 47 months) showed, first, generalization to size, then bidimensional generalization. (3) Four children (age range, 46–57 months; mean age, 50 months) showed, first, generalization to neither component, then bidimensional generalization. (4) Seven children (age range, 38–58 months; mean age, 47 months) showed, first, generalization to neither component, then generalization to size, and lastly bidimensional generalization.

III. DISCUSSION

The first general trend indicated by the results is that each child passes through the same generalization sequence in which, as training proceeds, there can, in turn, be removed without discrimination breakdown, first, *neither* of the two stimulus components, secondly, the width component, and lastly, *either* the size *or* the width component. It may be that

the above four groups result from the children's passing through genuinely different generalization stages, but it seems more likely that the performances of those children not showing all three stages can be accounted for in terms of the stages succeeding each other so rapidly that either, or both, of the first two escape detection. The experimental method employed could hardly be expected to sample every generalization stage through which every individual passes so that the failure to demonstrate a particular stage in any child does not at all mean that he does not pass through it. The absence of the first two stages in some subjects affirms that bidimensional generalization is not merely due to increasing familiarity with the test stimuli. On a more general plane still, these sequences of generalization types illustrate the basic developmental process of 'perceptual articulation', the process which Werner describes as 'the development from a diffuse perceptual organization characterized primarily by "qualities-of-the-whole" into an organization in which the essential feature is a decisiveness of parts standing in clear relationship' (Werner, 1940 pp. 111-12). Perceptual articulation plays a part in many discrimination situations, and Murphy and Hochberg go so far as to claim that 'progression from (a) homogeneous through (b) differentiated to (c) integrated characterizes, in principle, not only the stages through which the perceptual development of the maturing child passes, but also (with important qualifications...) the course of every perceptual process, regardless of age.' (Murphy & Hochberg, 1951, p. 334).

The second general trend indicated by the above results is that perceptual articulation is facilitated not only by increasing amounts of training but also by increasing chronological age. The children showing bidimensional generalization straight away are an average of nine months older than those who show, first, any of the more primitive types of generalization. This age difference is, however, no more than suggestive since there are too few subjects to permit statistical analysis. But the age difference is in the right direction (see below) and the writer believes that it would be worth seeing if it became larger if mental age were considered rather than chronological age.

The results also show that, with the particular stimuli used, the size characteristic is more readily reacted to than the width characteristic. There is not a single case of a child's generalizing to the width component before being able to generalize to the size component.

It is now time to return to the child's generalization behaviour in the transposition situation. As suggested in the Introduction, the training stimuli in that situation may be regarded as bivariate, as possessing both absolute and relational components. By analogy with the above suggested trends, with younger children and small amounts of training, the stimuli will give rise to a diffuse perceptual organization, and the alteration of either component will lead to discrimination breakdown, i.e. transposition will not occur except with test pairs which preserve the relational component and are, in their absolute characteristics, very similar to the training pair. On the other hand, with older children and large amounts of training, the perceptual organization resulting from the two components will become progressively more articulated and transposition will appear to test pairs increasingly dissimilar to the training pair.

Another effect not infrequently found in the transposition situation is that, consequent on the increased frequency of transposition with increasing amounts of training, further training leads to a decrease in transposition and a choice of the positively trained stimulus or of stimuli similar to it. This effect, too, can be viewed in terms of perceptual articulation if a third stimulus component is admitted which comprises the absolute charac-

teristics of the positive stimulus. Generalization in terms of this component is, of course, incompatible with generalization in terms of the relational component and less frequent transposition will result.

IV. SUMMARY

The generalization behaviour of twenty-seven children, of average age 50·2 months, is investigated in a bivariant stimulus situation. It is found that, when the discriminative reaction is newly established, removal of either stimulus component produces discrimination breakdown but that, with further training, generalization occurs to test situations which provide either component in the absence of the other. Results are fully consistent with the hypothesis that increasing amounts of 'perceptual articulation' occur with increasing amounts of training and, further, occur more readily the older the child. The concept of 'perceptual articulation' is used to interpret the generalization behaviour previously observed in the transposition situation. It is argued that the transposition situation may profitably be regarded as a special case of the more inclusive bivariant stimulus situation.

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THE EFFECT OF HUNGER UPON DRINKING PATTERNS IN THE RAT

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I. *Introduction* (pp. 294-295). II. *Method* (p. 295). III. *Results* (pp. 295-297). IV. *Discussion* (pp. 297-298). V. *Summary and Conclusions* (p. 298). *References* (p. 298).

I. INTRODUCTION

Although animals have been used extensively in behavioural studies involving thirst and hunger, attempts to study the actual interaction of these two complex drive mechanisms seldom appear in the literature. Siegel and his associates have published three studies in this area. Siegel & Stuckey (1946) investigated the relationship between water intake and food intake in the rat. Although their study was primarily concerned with the validity of Richter's hypothesis that there is a direct relationship between skin area and water intake in the rat, they reported (p. 273 n. 3) that their rats drank 29% less water when they fasted than when they were on *ad libitum* feeding. In another study the same authors (1947) found that the food and water intake of rats showed a marked correspondence when the animals were given food and water *ad libitum*. Siegel and Talantis (1950) found that depriving rats of food did not alter the relationship between water intake and hours of water deprivation.

Finger & Reid (1952) and Strominger (1947), working with rats, report that when rats are deprived of food, they voluntarily restrict their water intake.

Some earlier studies also attempted to investigate the interaction of thirst and hunger. Gregerson (1932), working with dogs, found that fasting usually reduced the 24 h. intake of water to one-fourth normal or less and that almost all of the water consumed by his experimental animals was drunk within 2-5 h. after feeding regardless of the time when food was given. Gregerson believes this is attributable to a temporary state of dehydration due to withdrawal of water from the body for the secretion of digestive juices. Kleitman (1927) reported that starving dogs showed a decrease of 65-80% in total water intake. In another study, using dogs, Robinson & Adolph (1943, p. 41) reported; 'It is shown above that dogs which were fed many hours ago drink little water. This fact has long been recognized in that dogs drink only one-tenth to one-third as much water upon days when food is withheld as upon days of usual feeding.' They further report on p. 43: 'It was found that water was usually drunk a few minutes after dry food was eaten. Since the reverse rarely occurred, some support could be deduced for the notion that eating sets off drinking. . . . The important point here is, however, that if the dog's five meals a day are stopped by deprivation of food, drinking that ordinarily follows each meal goes on just the same. The amounts drunk are reduced, but the same number of draughts are taken per day. Hence the pattern of drinking is demonstrated to exist independently of eating, though ordinarily the two work together in one sequence.'

The present experiment was undertaken to study quantitatively the effect of hunger and thirst upon drinking behaviour in the rat under varied conditions of food and water deprivation.

II. METHOD

Forty naïve albino rats, twenty males and twenty females, 90–120 days of age at the beginning of the experiment, were used in the present study. The experiment was run in two successive parts, each of which utilized ten males and ten females. All animals were on an *ad libitum* food and water schedule up to the beginning of the experiment. The drinking tubes employed were standard chemical laboratory cylinders, graduated to 1 ml. Accurate readings could be made to $\frac{1}{2}$ ml.

During part I of the experiment ten animals, five males and five females, were placed in individual feeding cages at 8 a.m. and deprived of food and water for 24 h. At the end of the deprivation period water was introduced. The drinking tubes were read after the following time periods: 2, 5 and 10 min., and 1, 12 and 24 h. Five of the ten animals were also given 35 g. of Purina Dog Checkers at the time that water was introduced.

The other ten animals, five males and five females, used in part I were treated exactly as the first ten except that they were started on the experiment at 11 p.m.

Part II of the experiment was a duplication of part I except that the animals were placed on a limited feeding schedule for 8 days prior to the 24 h. deprivation period. During this 8 day period the animals were each given 9 g. of Purina Dog Checkers per day, and water was available at all times.

The readings on the night animals of group II were taken 17 days after the last of the readings were made on the animals of group I.

III. RESULTS¹

Separate analyses of co-variance using body-weight as the correlated variable were run for each time of reading, since the variances for the different times of reading were not homogeneous. The analyses were run with respect to the following variables: sex, feeding schedule (FS) *v.* no feeding schedule (no FS), food and water *v.* water alone and night *v.* day.

At the 2 min. readings there were no differences with respect to any of these variables. At the 5 min. readings the water intake of the food and water animals (the FW group) was significantly greater (5% level of confidence) than the water intake of the water only animals (the W group). Differences with respect to the other variables were not significant.

At the 10 min. and 1, 12 and 24 h. readings the water intake of the FW group was significantly greater (1% level of confidence) than the water intake of the W group. The F-values were 8.36, 46.19, 146.45 and 265.40 respectively. None of the other variables achieved a significant F-value, and the F-values of these other variables were not ordered as were those for the FW and W groups.

¹ After the present study had been completed and the manuscript prepared for publication, a study by Verplanck & Hayes (1953) appeared. Since the present study presents some findings which correspond with the findings for the Ow (21½ hr. of food and water deprivation followed by water) and Ofw (21½ hr. of food and water deprivation followed by food and water) groups of the study of Verplanck & Hayes we prepared a chart to compare our 1 hr. readings after 24 hr. of food and water deprivation with theirs. The difference in deprivation time of the two studies prevents strict comparability of results.

	V. & H. mean	C. & B. mean
Ow	4.18	3.9
Ofw	10.05	11.0
w/fw	0.42	0.36

Thus, the values we obtained correspond quite closely to those obtained by Verplanck & Hayes.

Table 1 compares the mean water intake for the W group and the FW group for the different times of reading. It is seen in Fig. 1 that water intake is a negatively accelerated function of the time since water was made available, which corresponds with the findings of Stellar & Hill (1952). The asymptote for the FW group is considerably higher than that for the W group.

Fifty per cent of all the animals drank immediately regardless of whether food was introduced with the water.

Pearson product-moment correlation coefficients were computed separately for the W and FW groups. Of the six pairs of correlation coefficients so obtained only the pair

Table 1. *Mean water intake at the six times of reading for the water group and the food and water group*

Time of reading	Water group	Food and water group	Probability of significant difference
2 min.	1.6	1.2	—
5	2.1	2.8	<0.05
10	2.2	4.3	<0.01
1 h.	3.9	11.0	<0.01
12	7.1	32.3	<0.01
24	8.1	37.6	<0.01

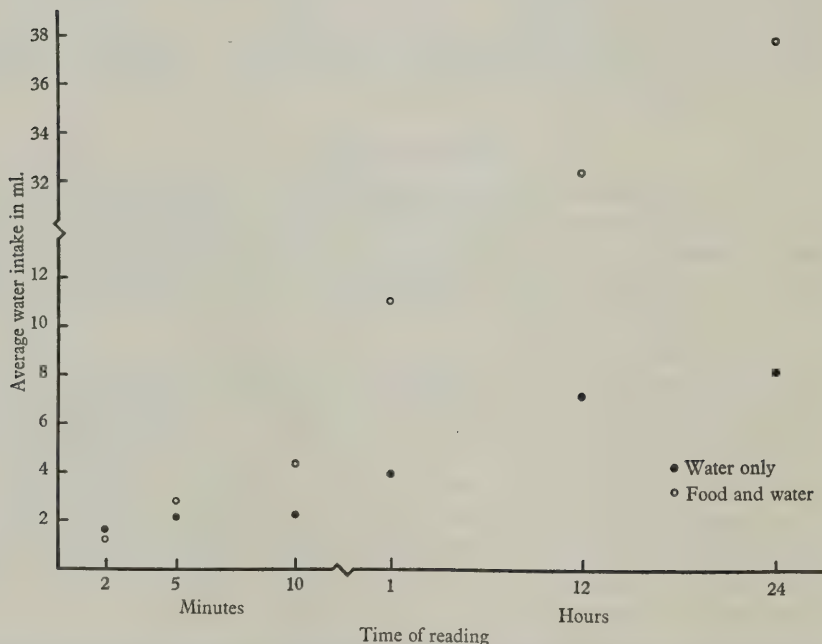


Fig. 1. Mean water intake at the six times of reading for the water group and the food and water group. ■

for the 2 min. reading was significant. Since the difference between any pair was less than the standard error of either member of the pair, the correlations were re-computed for the different times of reading with the data for the W and FW groups combined, thus increasing the D.F. to 38.

The correlation coefficients between water intake and body weight were as follows. For the 2, 5 and 10 min., and 1, 12 and 24 h. readings these were -0.44 , -0.34 , -0.31 , 0.17 ,

0.06 and 0.04 respectively. The first is significant beyond the 1% level of confidence, the second and third beyond the 5% level of confidence, and the last three are not significant.

The corresponding values of η for the regression of water intake on body weight are 0.60, 0.55, 0.52, 0.38, 0.29 and 0.19. The first four are significant beyond the 1% level of confidence, and the last two are not significant.

Deviation from linearity is significant in the case of the 2, 5 and 10 min. readings (1% level of confidence), and in the case of the reading for 1 h. (5% level of confidence). Deviation from linearity is not significant in the case of the 12 and 24 h. readings.

IV. DISCUSSION

The inverse curvilinear relationship between water intake and body weight found in the present study was a source of considerable surprise since Siegel & Stuckey (1946) found a low positive, but not significant, correlation between body weight and water intake when the food deprivation period was increasing from 0 to 23 h. and 45 min. It is to be remembered, however, that the animals of the present study had already been deprived of food and water for 24 h. before the drinking tubes were introduced.

Of further interest is the finding that the magnitude of the correlation between body-weight and water intake decreases as the amount of time that water is available increases, regardless of the presence or absence of food, up to 24 h. which was the limit in the present study.

The FW group drank a mean of 37.6 ml. of water over a 24 h. period as compared with 8.1 ml. for the W group. Siegel & Stuckey (1947) found that the average water intake for a 24 h. period with animals on an *ad libitum* food diet was 32.6 ml. Both of the values (8.1 and 37.6) of the present study are beyond the range of the true mean of the Siegel & Stuckey study. This may of course be attributed to the difference in experimental procedures mentioned earlier.

The fact that the present study gives no evidence of a differential water intake between night and day when animals are deprived stands in direct contradiction to results presented by Siegel & Stuckey (1947), Stellar & Hill (1952), Young, *et al.* (1952). We can offer no explanation of this contradiction, but can only point out that our rats were deprived of food and water for 24 h. before the test situation which may well have altered the drinking behaviour which was observed by the other investigators. Young, *et al.* (1952, p. 85) made a similar observation: 'From our records it is clear that a 6 hour deprivation from 6 a.m. till noon would have little effect upon the water intake of our animals inasmuch as they normally drink little during these hours. But an equal period of deprivation during the night hours might have a very different effect upon drinking behaviour, especially if the thirst drive were tested at night.'

Of equal interest was our finding that there were no significant differences between the FS and the noFS groups. Restricting the food intake of the animals of part II for 8 days prior to depriving them of both food and water for 24 h. had no effect on their water intake, for when their behaviour was compared with that of the part I animals, which had been on *ad libitum* feeding prior to their 24 h. period of water and food deprivation, no significant differences between the groups were discovered.

Our W group's drinking only about one-quarter as much water as the FW group is in agreement with the results of other investigators (Gregerson, 1932; Kleitman, 1927; Siegel & Stuckey, 1946). The finding of the present study that there is reduced water

intake in the W group tends to support the hypothesis that eating sets off drinking. Our finding that there is reduced water intake coupled with the findings of Stellar & Hill (1952) that rats drink at a constant rate or do not drink at all, allows the deduction that rats do not take the same number of laps when food is available and when it is not. Robinson & Adolph (1943) reported that while dogs took in less water when food was not available they still took the same number of draughts. Since Stellar & Hill counted laps and Robinson & Adolph counted draughts we are not really in a position to make a comparison and the difference may be explained simply on the basis of the variable considered.

V. SUMMARY AND CONCLUSIONS

Water intake of forty albino rats was measured after 2, 5 and 10 min., and 1, 12 and 24 h. under two conditions of food-deprivation. Group I consisted of twenty animals which were deprived of food and water for 24 h. Group II consisted of twenty animals which were put on a 9 g. per day food schedule for 8 days and then deprived of all food and water for 24 h. Half of the animals of each group were started during the day and half at night; half of each of these groups had food available during the period of water intake measurement and half did not.

Quantitative measures of the effect of hunger and thirst upon drinking behaviour in the rat under varied conditions of food and water deprivation are presented.

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PUBLICATIONS RECENTLY RECEIVED

The Nature of Prejudice. By G. W. ALLPORT. Massachusetts: Addison-Wesley Publishing Co. 1954. Pp vxiii+537. \$5.50.

Prejudice is possibly the most fully studied of all topics in social psychology, and there is no doubt of its great practical importance in this time of awakening nationalism amongst the coloured populations of the world and the anxieties accompanying that awakening in the advanced western peoples. Allport in this extensive review succeeds in demonstrating both these points, and, in these days of fashionable 'fundamental psychology' confined to the laboratory, shows that at least one branch of psychology has something reasonable and useful to say about human affairs and their conduct.

His approach is a model one. He first defines prejudice; a definition which is careful to include all forms of prejudging and which does not restrict itself to the more obvious forms of colour and ideological prejudice. Most of us are prejudiced in one way or another if we accept his definition and, as he implies, even the most liberal person may be violently prejudiced in some ways. The negrophil is often as prejudiced as the negrophobe. Not only are we all prejudiced but without a certain amount of prejudging of persons and issues we could not behave effectively; prejudice is a useful and a normal form of behaviour which can only be regarded as pathological when it leads to maladaptive behaviour and gross mistakes in estimating the nature of the environment.

This view of prejudice at once makes it a matter of character structure and Allport produces ample evidence that this is the case. He deals fully with the ontogenesis of prejudice and reviews fully all the studies on prejudice formation in childhood. The recent material of the Frenkel-Brunswick school occupies an important place in the book, together with the older studies of the personality structure of the prejudiced person. And Allport does not restrict himself; he gives full weight to the clinical and psycho-analytic studies as well as to the more quantitative and rigorous investigations. This catholic approach to the material is one of the most stimulating features of the book, and it is to be hoped that the custom of regarding all evidence as useful in a beginning science will become usual.

The emphasis on the character structure of the prejudiced person implies that the removal of prejudice is less a matter of instruction than of therapy. The later chapters of the book discuss the experiments already performed on the cure of prejudice and, so far as it goes, the evidence supports the need for special quasi-therapeutic techniques. But it is in this field that least has been done, and it is clear that there is a great need for more experiments on methods of destroying prejudice. It might be the case that if we had useful techniques our political tasks in Africa and the East would be made easier.

Allport does not make the common professional mistake of neglecting social and economic causes of prejudice. However, he does show that they operate by affecting the acts of individuals and regards the psychologist's task as done when he has explained the precise manner of their action on the character of the individual. He briefly but adequately reviews the work on the social and economic causes and shows their relation to the character of the prejudiced individual.

There is no better survey of the subject of prejudice, and this book is likely to become a standard text for social psychologists beginning the study of prejudice. It is spoiled a little by equal weight being given to hearsay evidence and to more certain material. However, it is easy to distinguish which is which in the text. Lastly, the book is very readable and can be easily understood by laymen. For this reason it should have a wider audience than the professional psychologist whose technical voice is rarely understood in political and administrative circles.

R. C. ALBINO

Tâche, Réussite et Échec.—Théorie de la Conduite Humaine. By J. NUTTIN. Antwerp: N. V. Standaard-Boekhandel. 1954. Pp. ix+530. Sewn, 280 Belgian francs; bound, 330 Belgian francs.

The subject of this massive work by Professor Nuttin is the interplay between motivation, past experience and present apprehension in the determination of adaptive behaviour and the development of personality. Its scale matches appropriately the breadth of the theme, and it incorporates a great quantity of experimental work, for the most part hitherto unpublished. The empirical material is sustained by most careful discussion, explicitly based upon the fullest study of contemporary and recent work in relevant fields.

It is Nuttin's contention that no simple hypothesis of temporary need-reduction, even when supported by a conception of secondary motivation, can fully compass the motivational aspects of learning and performance as they are observed in human subjects. He has conducted a series of very ingenious experiments, for the most part concerned with the effects of apparent success and failure upon subsequent recall both of experimental material and of success or failure themselves. These lead him to the conclusion that performance, while it arises within a frame of existing needs, leaves behind it not merely the *reduction* of a need, but also the *persistence* of a need. This later becomes incorporated in a stably evolving system of orientation, at once cognitive and dynamic, in the individual. The need-reduction aspect of successful performance has, thus, far less importance from a broad psychological point of view than that attributed to it by many behaviouristic theorists. What is of much greater interest is the manner in which success or failure contributes to the persistence and evolution of the individual's motivational system. For it is through this effect that fresh ways of regarding the environment, and of developing expectations, significances and potential satisfactions within it, are engendered. And so in turn the means of bringing past experience and established habits to bear in novel adaptive reactions are developed.

No brief review could do justice to the elaborateness with which Professor Nuttin has developed this theme. He has, in effect, made an outstanding effort to weave into a unified theoretical formulation all the manifold considerations and contradictions afforded by such diverse approaches as those of Thorndike, Clark Hull, Tolman, the Gestalt School and Bartlett. The meal is large, rich, varied and most skilfully prepared, if a trifle difficult to digest. One is left a little uncertain, however, whether the basic materials really lend themselves to this kind of *liaison*.

R. C. OLDFIELD

The Structure of Human Personality. By H. J. EYSENCK. London: Methuen. 1953.
Pp. 348. 37s. 6d.

In this book Dr Eysenck gives a thorough and sometimes sharply critical account of a large number of studies, mainly factorial, of various aspects of personality, using rating methods, questionnaires, objective behaviour tests, measurements of physique and physiological measurements, and assessments of interest and attitude. He shows that in the main the results of these studies are compatible with the 'dimensions' or principles of classification which he has set forth in previous work. He also shows that there are fewer differences between the results of studies using different factorial methods than might be thought, especially when second-order analysis is done on oblique primary factors. One would not deny that factorial methods can be of considerable value in enabling us to clarify and improve our classificatory categories. Many, however, would not agree with Dr Eysenck that the value of typologies or systems of classification can be established only by factorial methods.

Dr Eysenck lays stress on the use of the 'hypothetico-deductive method'. But his use of the term 'hypothesis' appears to the reviewer somewhat doubtful. For example, he claims to test 'Jung's hypothesis that the conduct of extraverts is more determined by objects and relations in the external world, while the actions of introverts are more determined by their inner subjective states' (p. 29). He uses the 'level of aspiration' technique, and finds that introverts (represented by dysthymics) aspire higher than they perform, and perform better than they estimate, while the reverse is true of extraverts (represented by hysterics). This is taken as supporting the hypothesis. Now, assuming that one accepts Dr Eysenck's selection of his two groups, the question is whether the hypothesis is a genuine hypothesis in the sense that it could be disproved conclusively by experimental procedure. One might legitimately argue that any experimental procedure such as Dr Eysenck's is not unambiguously relevant to the hypothesis, so that a critical experiment would not be possible. To put it in another way, all that one could conclude from a negative result would be that the behaviour referred to by the terms 'introvert' and 'extravert' does not include the behaviour in the experimental situation. This is the same kind of point as that made by Albino in his recent article in this Journal. It does not, of course, imply that it is not interesting and valuable to endeavour to define or extend the range of classificatory categories.

A few other points might be made. For example, one would like to know how it comes about that rating of obsessional tendencies has a zero-loading on a factor of 'neuroticism' in one study (p. 55), while in another study involving subjects classified as cases of psychopathy, obsession, hysteria, anxiety state or post-traumatic personality change, 'the first component (severity of neurosis) ranks the groups in order from normal to obsessional and psychopathic' (p. 57).

Such questions, however, should not be allowed to detract from the principal merits of the book, which should be read by all interested in the assessment of personality.

D. GRAHAM

The Origins of Psycho-Analysis. Letters to Wilhelm Fliess, Drafts and Notes; 1887-1902.

By SIGMUND FREUD, edited by MARIE BONAPARTE, ANNA FREUD and ERNST KRIS.
London: Imago Publishing Co. Ltd. 1954. Pp. xi+486. 30s.

This book contains a selection of intimate letters written by Freud during the period of his greatest originality of thought to Wilhelm Fliess, a biologically minded physician in Berlin, under the influence of a friendship which evidently provided Freud at a critical phase with the inspiration to pursue his momentous researches undeterred by difficulty, discouragement and disappointment. Fliess was an aberrant genius who came to harness the concept of bisexuality to a pseudo-scientific system of periodicity based on female menstruation. His growing extravagance of thought and intolerance of disagreement ultimately led, in 1902, to an alienation of Freud's friendship; but the emotional basis of the friendship had already become undermined by the arduous self-analysis commenced by Freud in 1897. The influence of this self-analysis upon the development of Freud's psycho-analytical theories, e.g. his theory of dreams, is also reflected in the letters—as is the conflict (never wholly resolved) between his ambition to provide a purely psychological explanation of psychopathological phenomena and a lingering desire, encouraged by Fliess' influence, to achieve an explanation in physiological terms. A remarkable wealth of theoretical material is combined in the letters with interesting personal detail; and the drafts accompanying certain letters contain material never embodied in Freud's published works. The book will provide the student of psycho-analysis with a fascinating study; and for the intimate light which it throws upon the manner in which a scientific genius works it is almost unique in literature.

W. RONALD D. FAIRBAIRN

Psychoanalysis as Science. By ERNEST R. HILGARD, LAWRENCE S. KUBIE and E. PUMPIAN-MINDLIN. Stanford University Press; London: Geoffrey Cumberlege. 1952. Pp. x+17. 34s.

Psychoanalysis and the Unity of Science. By ELSIE FRENKEL-BRUNSWIK. Proceedings of the American Academy of Arts and Sciences, Vol. LXXX, No. 4. 1954. Pp. 271-350. \$1.55.

Psychoanalysis as Science consists of five lectures delivered at the Carnegie Institute of Technology, 'to acquaint an audience of scientists from other fields with the basic principles underlying, and the basic evidence for, psychoanalytic concepts'.

In the first two lectures, Dr Hilgard employs his outstanding powers of exposition in summarizing the attempts that have so far been made to subject Freudian hypothesis to experimental tests. The lectures confirm Freud's own prediction that 'the future will probably attribute far greater importance to psychoanalysis as the science of the unconscious than as a therapeutic procedure'. They contain some important material on experimental neurosis in human beings and animals, but no reference is made to attempts to measure the success, or otherwise, of psychoanalysis in the treatment of real life neurotics. On the other hand there is an impressive array of experimental evidence for the validity of the basic Freudian concepts, particularly the defence-mechanisms and dream symbolism. Some of the material was summarized in Sears' 1944 survey, but much of it is more recent. Of special interest is the full account of Keet's 'micro-analysis', originally described in *Psychological Monographs*, 1948.

Dr Hilgard himself is, of course, by no means a Freudian. He sums up his theoretical position by saying, 'I do not care whether we end up believing that Freud was a scientist or a romanticist. If eventually we are able to make science out of materials that he called strongly to our attention, then we shall owe him that historical debt, whatever the verdict may be about his own formulations.'

In the third and fourth lectures Dr Kubie, a Professor of Psychiatry and a practising analyst, questions the value of providing 'pallid laboratory facsimiles' of facts that have already been abundantly proved in real life; and he gives a string of examples—some from everyday experience and others from clinical practice—where, in his view, the Freudian implications are too obvious to reject. One or two of these examples are open to scepticism, but most of them are convincing and striking: they must have considerably startled an audience new to the subject.

In Dr Kubie's view, the function of experiment in psychoanalysis is to advance knowledge, not to convince those who are 'reluctant to look human nature in the eye': and he sketches an ambitious programme for the future, which includes, first, the foundation of a research institute in which psychoanalysts would collaborate with clinical psychologists, neurologists, biologists, sociologists and statisticians, and, secondly a long-term research project involving the co-operation of 'a statistically adequate sample of young couples in some relatively stable community'.

In the final lecture (which might more logically, perhaps, have come first) Dr Pumpian-Mindlin, a psychoanalyst and the editor of the series, gives a lucid account of the historical origins and development of psychoanalysis and its relation to sociology and biology.

Psychoanalysis and the Unity of Science deals with the operational status of psychoanalytic concepts; Freud's theory of instinct, with special reference to the vexed question of 'alternative manifestations' (the aspect of Freudian theory which has led Eysenck to say that it can explain anything but predict nothing); the verification of psychoanalytic hypotheses; the bearing on psychoanalysis of 'perceptual rigidity' and other aspects of cognitive psychology; and the relation between psychoanalysis and ethics. As a monograph it is perhaps a trifle formless, and some of the semantic discussion is heavy going; but, like everything written by this author, it abounds in penetrating insights and constructive ideas.

MARGARET KNIGHT

The Analysis of an Obsessional. By R. W. PICKFORD. London: The Hogarth Press. 1954. Pp. xii + 234. 21s.

Dr Pickford is one of the very few psychologists in this country who combine distinguished work in the academic field, notably in social and experimental psychology, with a very full involvement in clinical psychology and psychotherapy. In this book he uses the detailed records of an analytic form of therapy with a patient, seen about once a week over eighty sessions, to demonstrate the clinical functions and scientific status of the method and to illustrate a good deal of its theoretical basis. It is not presented as a typical case study, but rather because of the 'interesting fantasy material produced by the patient, and the clear way in which the details of the illness and its causes and treatment revealed themselves'. A large part of the material considered consists of most interesting sequences of dreams.

This book will be criticized by members of the orthodox schools of psychoanalysis on account of the technique employed in many instances, the use of transference in the analysis, and indeed on some points of theoretical approach. The title of the book and its diagnostic label may focus some of the criticisms. But if the work reported is viewed more as an adaptation of psychoanalytic technique to the somewhat short term and non-intensive (i.e. only about once per week) possibilities in the case, Dr Pickford's method raises many fruitful issues which perhaps are of more importance than the probable points of criticism; for example, the possible advantages of shorter and less intensive work with certain kinds of patients, the need to understand far more about the dynamic structure of personality as a basis for planning treatment, and, what the author so well illustrates, the importance of clarifying why the analyst employs a particular technique in the therapy as a whole as well as in the precise situations that occur in its progress.

This reviewer finds the earlier chapters, in which more theoretical discussion is introduced, less satisfying than the more clinical descriptions. Dr Pickford's analyses of the dream sequence are particularly illuminating. The very simple style of writing belies his unusually sensitive insight into the psychodynamics of association and symbolic meanings.

In a brief review it is not possible to draw attention to more than a few of the valuable clarifications offered in this book. There are very good illustrations of the layering and interweaving of phantasies from different levels of personality development, such as are classically ascribed to the oral, the urethral and anal, and the genital phases. There is evidence of the way in which unconscious phantasy meanings influence all human endeavours, including the narrowly scientific. For the clinical psychologist there are many relevances to the use of projective methods of personality assessment; some of these are pointed out by the author, many others are clear throughout the book.

It is doubtful how successfully Dr Pickford has achieved his aim in demonstrating the clinical function of a psychoanalytic method as well as its scientific status. But only those critics from the 'scientific' opposition who have abnormally strong resistances will remain unmoved by his efforts.

H. PHILLIPSON

Dreams and Nightmares. By J. A. HADFIELD. Harmondsworth, Middlesex: Penguin Books Ltd. 1954. Pp. xii + 234. 2s. 6d.

It is recorded that two thousand years ago there were in Jerusalem twenty-four professional interpreters of dreams whose diverse interpretations of any dream were all equally correct. Dr Hadfield's skill in dream interpretation would, I feel sure, exceed their combined efforts. He gives a brief account of popular, semi-popular and unpopular explanations of dreams, physiological ('heavy supper'), physical, and

premonitory theories, theories in terms of personal or racial reminiscence, and the celebrated Freudian, Jungian and Adlerian theories, and he finds a grain or more of truth in all of them. Selecting what he regards as their most reasonable features and adding something significant of his own, he advances a new explanation, which he calls the Biological Theory of Dreams. The characteristic of dreams, he holds, is their problem-solving powers. The beginning of the dream tells us the nature of the problem confronting the dreamer and perhaps its cause, and the end of the dream suggests a solution.

This theory, well presented though it is, would carry more conviction if we could assume that the dreamer usually knew the meaning of his dreams. This is indeed what Dr Hadfield claims, for he tells us that the dreamer 'can sense the significance of a dream intuitively although he may not be able to understand it cognitively' (p. 97). Among his many examples is one from a patient who dreamt that she was walking down Regent Street with no clothes on. 'By making her exhibitionistic, but telling her it does not matter, the dream quite automatically gets rid of her embarrassment and restores her self-confidence; the dream does this independently of any interpretation, merely by her undergoing the experience.'

But surely it is not enough merely to *assume*, as Dr Hadfield seems to do, that we have an intuitive understanding of our dreams. We require some evidence that this is the case.

The book is clearly and attractively written and embodies considerable experience and many shrewd observations. It will doubtless be thoroughly enjoyed by a large lay public. The critical student, however, is not likely to feel satisfied either with the chapter on nightmares or with that on dreams and psychical research. In particular he will find the discussion of neurosis and dreams inadequate for it is not based on a coherent account of infant development.

There is an error to be corrected: Heffer is the publisher not the author of *The Anatomy of Inspiration*, written by R. F. M. Harding.

JOHN COHEN

Mental Health and Human Relations in Industry. With a Foreword by Lord Horder.

Edited by T. M. LING. London: H. K. Lewis and Co., Ltd. 1954. Pp. xvi+268. 21s.

Of the many institutions offering training in 'human relations', the Roffey Park Institute justly holds the highest place; clinical experience, research, and a wide choice of skilled lecturers have given it an international reputation for its teaching about the psychological, social and medical problems of industry.

Now, the key points of that teaching have been gathered into the present book (it is also being published in America, and translated into ten languages from Arabic to Serbo-Croat-Slovene). Under the able editorship of Dr Ling, who contributes the introduction and four of the subsequent chapters, the book presents the substance of the teaching of some of those who have lectured at the Institute—it can be the substance only, because the wide scope of the book does not allow many of the points to be fully developed. Yet even this is largely overcome by excellent indexing, full references for each chapter, and unobtrusive cross-references between chapters; the result is a first-rate summary and source book, and a fine example of interdisciplinary teamwork, for the authors include two psychologists and two psychiatrists, two industrial medical officers, an industrial sociologist, an industrial education officer, and a former personnel manager. As Dr Ling says in his preface, 'the composition of this team symbolizes the way in which social problems in industry are being considered today'. This partnership is very happy, and an example of the value of such co-operation, with its mutual respect for different approaches to the same subject. It is not easy to single out individual chapters (and all are of a very high standard anyway), but of particular interest to psychologists will be V. W. Wilson's two chapters (on the individual and the group in industry, and on the importance of selection)—both saying much, clearly, in little; and H. G. Maule's succinct chapter on the working environment. N. C. Rimmer, in the last chapter, sums up well the main implications of the book for management.

This book, clearly written, free from jargon, readable, and above all, of topical importance, is one which should be a well-thumbed prize on the bookshelf of any industrial psychologist, or indeed, of anyone concerned with human problems in industry, which, as the book rightly says, is 'an inescapable responsibility of all grades of management'.

T. M. HIGHAM

Industrial Training for Manual Operations. By W. DOUGLAS SEYMOUR. London: Pitman, 1954. Pp. xii+203. Price 20s. net.

This book is addressed to managers and supervisors and has two aims: (i) to give a comprehensible outline 'of the mental and bodily processes involved in the acquisition of skills'; (ii) to provide them with a 'guide book on how to set about training workers on repetitive operations'. The second aim is brilliantly

achieved. It is not that the first aim is not achieved: it is rather that the chapter on the theoretical basis of the acquisition of skills seems irrelevant to the rest of the book. For example, the essence of Mr Seymour's contribution—or rather the basis on which his system of training is erected—is this: repetitive tasks should be trained by the 'part' method, the training being carried out at the speed of the experienced piece-rate worker; the parts are fused into pairs, then trios, then, by way of training of 'overlaps', on to the 'wholes'. As far as I can see, no light is shed on any of this by Chapter II, the theoretical chapter. In fact, the problems of wholes versus parts, or slow versus fast—these are not faced at all. It is, indeed, rather unfortunate that Mr Seymour was persuaded to insert this theoretical section: it smacks of the text in *Education Psychology* in which the author dutifully parades Thorndike's cats, Pavlov's dogs, Munn's rats... and Old Wolfgang Köhler and all—thinking apparently, that one must do all this before getting on with the job of showing intending teachers how to run free activity in the infant school or of telling them not to hit small children if they get their sums wrong. Mr Seymour does, indeed, warn the ordinary manager and supervisor that he can ignore this section.

The main body of the book begins with a brief, but first-rate chapter on the analysis of operator skills giving all the warnings to the amateur on the nature and amount of material required for a satisfactory analysis. The next four chapters make an excellent detailed guide to the preparation of a training course: analysis, breaking down the operation into parts, training devices for these, synthesis of the parts. There are some good tips on inspection, with emphasis given to the difficult problem of quality specification. The training devices are well illustrated with photographs which really tell you something. Then follow separate chapters on the Training Centre, its staffing, the training of instructors, organization and supervision of a training centre (an excellent record card for trainees will be worth the cost of the book to many firms) and re-training of older and 'experienced' workers.

The last chapter, called 'The Results of Operator Training and Re-Training' is a restrained claim that training pays. Mr Seymour acknowledges that most of the results he quotes are from investigations of any efficient training versus just-picking-it-up. One instance is, however, modestly but convincingly recorded of a factory where Mr Seymour's methods could be compared with an existing training scheme.

The 22-page appendix reproduces a detailed plan of a training course in seamless linking, again with good photographs. I shall look forward to Mr Seymour's next book, *The Nature and Acquisition of Industrial Skills*, which is now in preparation. My guess is that, given the scope of a complete book, he should be able to do something which still remains to be done: bridge the gap effectively and convincingly between the experimental psychologist and the hugger-mugger of the industrial world.

DENIS MCMAHON

The Sociology of Work. By T. CAPLOW. Minneapolis: Minnesota University Press; London Geoffrey Cumberlege. 1954. Pp. x+330. 40s. net.

This is an extremely good book, with virtues lacking in the bulk of American writings on social science. It is brief, direct, and has a sense of form and proportion. It is also very attractively produced; the notes are well disposed, and the bibliography first-rate, apart from the usual inadequate coverage of relevant British and European work.

Professor Caplow has been a pioneer in the teaching of occupational sociology at the University of Minnesota. In the intervals between lectures and classes, besides getting through a vast amount of reading, he has found time to give advice to large firms on their administrative problems. After reading this book, one feels sure that he gives good advice, far removed from the flabby generalizations that often pass for 'human relations'. His approach is comprehensive; embracing mobility and status, conditions affecting the choice and assignment of work, union organization, and occupational beliefs and stereotypes. (The main difference between industrial and occupational sociology seems to lie in the range of occupations covered: Caplow ranges from beauticians to bootblacks, and cartoonists to cesspool cleaners.) There is a particularly interesting chapter on the occupations of women, in which the kinds of insights familiar through the work of Mead and Riesman are supported by more references and hedged around with more qualifications than usual. Although one finds frequent anthropological and historical excursions, the major emphasis is upon American society; a formidable enough topic in itself, of course. The major omission is a study of management comparable with the treatment of labour problems and union organization. Unfortunately, when managers adopt the open-door policy and let in the social investigator they open the door to the works rather than to the back office. Readers who want to find out more about the organization and beliefs of American management must turn to such studies as William H. Whyte's *Is Anybody Listening?*, a sprightly collection of articles by the assistant managing editor of *Fortune*.

A weakness of many American works on occupational sociology, especially those in the Mayo tradition, has been the avoidance of fundamental problems of power relationships, with all their political implications. Caplow does not make this mistake; he attempts to give due weight to coercion and control in the occupational system. The theme of control, rather than coercion, runs through much of the book, possibly because most of Caplow's experience has been with large-scale organizations which have a considerable bureaucratic element.

In one chapter, an important part of the social life outside the occupational field is examined. The strains placed upon modern families by their sharp separation from the highly specialized, impersonal, rapidly changing economic order are familiar to us all, not least through personal experience. In this study of the problems involved, the material is well ordered, and a new look given to housewifery by subjecting it to a job analysis and comparison with other occupations. It is found to be a queer occupation indeed, with its own frustrating dead ends and a reward system of which the guiding principle seems to be 'the more work, the less pay'.

The publishers believe that *The Sociology of Work* will be found useful as an introductory text, in addition to providing a handy source of information for research workers. Both these claims seem reasonable. But the American text—even when it has as many passing references to other societies as this one—has obvious disadvantages when used outside America and one hopes that Professor Caplow's work will soon be complemented by detailed and comprehensive studies of the division of labour in other countries. There is no lack of raw material.

J. F. MORRIS

The Social Psychology of Industry. By J. A. C. BROWN. Harmondsworth, Middlesex: Penguin Books Ltd. 1954. Pp. 307. 2s. 6d.

This readable book deals with relations within and between groups, and anybody who is interested in the human problems of industry will find it both instructive and stimulating. The emphasis is naturally on the social aspects of work and the author begins by tracing the changes that have taken place from medieval to modern times and then considers human nature and society, the formal and informal organization of industry, the nature and conditions of work, leaders and leadership, and frustration. He foresees that 'mass production techniques will be increasingly used all over the world and that the large enterprise, nationalized or otherwise, will grow in importance' and he believes the solution lies in 'the ability to adapt the new technological organization of industry so that it corresponds more closely with human needs'. According to Dr Brown, these needs are the desire for status, appreciation and emotional security; and all other factors, such as wages, welfare and working conditions, are of secondary importance. The book is intended primarily for the factory manager, the personnel manager, the time and motion engineer and the interested layman and it is commendably free from technical jargon. Some of the statements are rather too dogmatic and some of the supporting evidence is of doubtful validity; it needs time and patience to separate the substance from the shadow. Yet most people will agree with Dr Brown's diagnosis of the ills that industry is heir to though they would doubtless like to know just what should be done to cure these ills.

S. W.

The Social System. By TALCOTT PARSONS. London: Tavistock Publications Ltd., 1952. Pp. xviii + 575. 42s. net.

A number of American anthropologists, psychologists and sociologists (Professor Parsons among them) met at Harvard in 1949 with the grand design of outlining the basic principles in the study of social relations. Two years later there appeared the first-fruits, the joint work entitled *Toward a General Theory of Action*; and, as its title suggests, the fundamental concept is that of action. By this is meant the behaviour of interdependent persons 'oriented to the attainment of ends in situations, by means of the normatively regulated expenditure of energy'. It follows that a general theory of action must have three components: a theory of personality focused on the individual actor, a social theory dealing with the relations of actors to one another, and a theory of culture concerned with the regulating process of cultural patterning. In *The Social System* Professor Parsons sets out to meet the second of these needs by making 'an attempt to present a logically articulated conceptual scheme' for the analysis of societies. The main body of the book falls into two parts. The first outlines and elaborates a scheme for the analysis of social structure in terms of five pairs of contrasted 'pattern variables'. The second deals with the motivational processes in social interaction and the relations of the social system to the cultural tradition. For good measure the book ends with a chapter in which the analytic scheme is applied to the American

medical profession, another on social change, and one in which the spheres of interest of the various social sciences are defined.

Classification seems to be a ruling passion in Professor Parsons, but whether he will go down in history as sociology's Linnaeus is mainly a question for his fellow-sociologists to answer in due time. Certainly his taxonomic scheme will have to pass an extensive empirical testing before gaining general acceptance as the best social typology available, and before that can happen it will have to be considerably elaborated, he says. But although his suggested analysis of social structure is probably his chief contribution to the general problem, the book by no means neglects functional aspects. The debt to Freud is unmistakable here, the treatment of motivational processes being a telling adaptation of psychoanalytic theory. Implicit in it, too, is an engagingly moderate version of field theory.

This approach to the dynamic aspects of social relations should by itself entitle the book to an audience among psychologists, but two considerations threaten that audience's size. Professor Parsons freely admits that his theory is still fragmentary and rapidly developing. The temptation to wait and see what comes of it all is understandable, therefore, but it would be a pity if psychologists failed to join in this ambitious combined operation by following the lead given by Gordon Allport, Murray, Sears and Tolman, all of whom contributed to the original discussions at Harvard. The other trouble is that Professor Parsons has a prose style which reduces a reader to feelings of exasperated illiteracy. Admittedly his subject is intrinsically complex and abstruse, but his way of expounding it aggravates this inherent difficulty to the point of setting up a yawning communication gap. Happily, although much of *The Social System* is a forbidding jungle of words, its penetration has been made much easier by Professor W. J. H. Sprott, who provided an invaluable guide-book in his review article in the *British Journal of Sociology* (vol. III, no. 3. 1952). With that help even those whose interest is merely in seeing psychological theories of learning, motivation and personality in broader perspective than usual will find much that is rewarding and challenging in such chapters as those on socialization and social control. To social psychologists in particular, floundering for want of theoretical orientation, the entire grand tour could be worth the effort involved.

A. J. FLOOK

The Study of Culture at a Distance. Edited by MARGARET MEAD and RHODA MÉTRAUX.
Chicago: University Press; London: Cambridge University Press. 1954. Pp. x + 480.
37s. 6d.

American social research is having a tremendous boom, thanks to the Office of Naval Research and the various Foundations. Many impressive and interdisciplinary projects are in progress, and many thousands of pages of reports are being published.

This volume is a collection of papers by Mead, Métraux, Bateson, Gorer and others—the staff of a project organized by the late Ruth Benedict at Columbia, and sponsored by the O.N.R. The aim of the exercise was primarily to study the culture and national character of countries behind the 'iron curtain', with an eye to political and military applications of this information. This book reports the methods devised for the 'study of culture at a distance', and gives many examples of the kind of results obtained.

The methods reported leave much to be desired from a rigorous point of view, though no doubt they can lead to results of a preliminary and suggestive nature. The principal method is the intensive interviewing of emigrants. Despite careful internal checks on the stories told, and the attempt to obtain informants from varied backgrounds, the suspicion remains that only certain kinds of people do emigrate, and that the distorting processes of memory, sometimes after 45 years, may prejudice the results. The charge of biased sampling applies even more to the use of projection tests, quite apart from the fact that these have never been shown to be cross-culturally valid. Another method is the analysis of literature and films. An obvious improvement on this would be to study only the most popular productions, otherwise the results may be quite unrepresentative. Furthermore even when the popular art of a society has been analysed, what does this tell us? Are slap-stick films a reaction to or a reflection of the normal life of a society?

Several of the underlying ideas of this group of writers seem somewhat unsatisfactory. For example a convincing case is not made out for the usefulness of the concept of 'national character' as applied to modern societies: no one has ever fully sampled the whole range of personalities present in one such society, or compared the different distributions in two of them. Again much reference is made to a supposed integration of 'psychoanalysis, anthropology and learning theory'. This is wishful thinking: each of the three comprises different sorts of data and different kinds of theory, and a genuine integration would entail very much more actual knowledge and a complete recasting of the three 'theories' in question.

The volume is very long, has many quotations from and references to previous works by the authors (including fifty by Margaret Mead), and contains extended excerpts from interviews and stories. This is all very interesting and suggestive, but should not be confused with the results which can be obtained by the use of those more sophisticated methods now known to social scientists.

MICHAEL ARGYLE

International Review of Criminal Policy, no. 3, January 1953. New York: United Nations. ST/SOA/Ser. M/3, 28 July 1953. Pp. 162. 15s.

Too many United Nations documents are insufficiently noticed. This is yet another excellent product, a special issue on the proceedings of the European seminar in Brussels in 1951 on the medico-psychological and social examination of offenders.

The seminar applied itself to describing the scope and objectives of such examination. It reviewed the various types of examinations required, and it studied the question of an offender's individual rights. This question dominated the study of the problem of the communication of experts' reports to the court and the question of professional secrecy.

In nineteen technical papers the subjects dealt with include the selection of persons for examination, the legal and procedural problems, biological and physical examination, psychiatric examination, social examination and the relationship between scientific examination and treatment.

Nineteen national statements describe the existing practice under the headings of methods of scientific examination of offenders and of inquiry into their social background, the function of scientific examinations and social investigations in judicial procedure, and the treatment and rehabilitation of convicted persons.

Many of these papers and national statements, and most certainly the conclusions of the seminar, which are thoroughly comprehensive and well-balanced, should be digested by everyone working with offenders. The material studied ranges from the biological and somatic examination of offenders to the moral and psychological implications of the scientific examination of offenders. All the papers are lucid, exact and very readable.

J. D. W. P.

The Young Delinquent in his Social Setting. By T. FERGUSON. Published for the Nuffield Foundation. London: Geoffrey Cumberlege, Oxford University Press. 1952. Pp. xii+158. 10s. 6d.

The main function of this book will be its value as an antidote to loose thinking and writing on the subject of juvenile delinquency. Professor Ferguson has filled in the background of our knowledge of the young delinquent by a most thorough and penetrating investigation. He does not claim to have made any advance on, say, Burt's ideas of causation, but to have produced a balanced study of the factors influencing juvenile crime. In all 2139 boys have been studied. The results have been tabulated in 79 tables and 21 figures. This gives some indication of the scope of the work and the care with which it has been analysed.

Perhaps two conclusions might be mentioned, as they are on topics which are receiving much emotionalized attention at present. On p. 145 Professor Ferguson says: 'The fact that the mother is employed out of the home appears to exert little influence on the incidence of delinquency.' Again on p. 145 and 146 he says that among the ordinary and physically handicapped boys: '...The fact that a home was "broken" has, not, in itself, constituted a major factor in the production of delinquency.' The mentally handicapped were slightly more dependent on a full complement of parents. Again he says: 'The presence of a bad parent in the home is less to be preferred than his absence from it.'

In general the incidence of delinquency was considered in relation to a number of social variables. 'Any one of these adverse factors by itself is bad enough; but the main danger springs from a co-existence of adversities which, together, may make it difficult indeed for a lad to keep out of trouble.'

This book should prove a most useful source book for further research on juvenile crime. It is a most able illustration of the type of quantitative analysis which must be done on this type of problem before the deeper qualitative work can be attempted.

MADELINE KERR

L'Enfant Instable. By ANDRÉ P. L. BELEY. Paris: Presses Universitaires de France. 1951. Pp. 43. 240 fr.

Using the method of case history, Dr Beley gives in this little book an account of eighty-four unstable boys, aged 8-20 years, for the most part delinquent, whom he saw for diagnosis and treatment at a psychiatric centre. In the Introduction he briefly discusses the concept of instability, enumerating some

of the most important definitions of it. He makes it clear that he defers his own definition of the term until he has presented his data, since he wishes to do that without preconceived ideas. He is concerned with boys of eight and upwards, since instability up to the age of seven years must be regarded as normal: later than that, it is pathological. He considers that pathological instability constitutes a serious problem in modern society, since one child in four suffers from it. Such instability can be characterized by features which are either mainly physical or mainly psychological, or it may present a combination of both. It is characterized on the physical side by such signs as hypermotility, tremors, clumsy movements, tics and the like, and on the psychological side by hyperemotionality, lack of emotional development, inconstancy of thought and purpose, which may arise from conditions primarily within the child, or it may be induced by unsatisfactory conditions within the family, leading to feelings of insecurity in the child.

The eighty-four cases in his sample were studied by means of a thorough physical examination, psychological tests such as the Binet, the Rorschach, the T.A.T., level of aspiration and attention, and by means of a psychiatric examination. Their general behaviour was also under close observation, and a full family history was taken. On the basis of the data so gathered, Dr Beley was able to divide his cases into the following broad types: (i) instability of physical origin and manifesting itself in physical symptoms; (ii) instability where character involvement is present. This second group is by far the largest, fifty-six out of his eighty-four boys falling into this broad category, which is further subdivided into ten groups. The principle of classification adopted by Dr Beley is not always very clear, and it is difficult to gather from the illustrative data presented why Dr Beley should have included in this group such heterogeneous cases as the hyperkinetic, the mentally backward, the schizoid, the paranoid, and the epileptoid; (iii) instability where there appears to be no connexion between the physical status of the child and any disorders of behaviour, and where the origin seems to lie in family conditions.

Dr Beley has undoubtedly been able to gather together a great deal of information of interest, but a different presentation of results would have allowed the ordinary reader to get a clearer picture of the conditions he describes. He gives short case histories of thirty-eight of the cases selected as being the most representative, but gives no tabulated information about the remaining cases. For instance, their ages are not given. Only six of the thirty-eight cases cited are below the age of twelve years; and it is clear that much of the investigation was concerned with the problem of the unstable adolescent rather than the unstable child.

Although a number of psychological tests appear to have been used, only the results of the Binet are given (and not in every case), and then only in terms of mental age. Scatter is mentioned as characteristic of the majority of cases, but no details are given. Further, from examination of the mental ages supplied, it seems clear that the majority of cases are of poor intelligence. Whether this is true of the sample as a whole it is impossible to say.

The other data presented are given in an inconsistent manner. Family heredity and details of family background are given in some cases and not in others. This makes comparison of the cases cited difficult, and one is given no help, since even the simplest quantitative treatment is omitted. Although the sample itself is quite large, it will scarcely stand Dr Beley's technique of subdivision. For example, in some instances only one or two cases fall into each subdivision of his second broad class, and no deductions have been or can be made as to their significance.

There are a few words of summary at the end of each section dealing with particular types of instability, but the general summary and conclusions at the end of the book are vague, and there is little attempt to discuss the concept of instability in the light of his own investigations.

It seems clear that considerations of space must have forced Dr Beley to be unduly selective in his presentation of the material he has gathered. This is to be regretted since his careful methods of observation and testing must have yielded a great body of interesting data.

GERTRUDE KEIR

The Psychopathic Delinquent and Criminal. By GEORGE N. THOMPSON, M.D. Illinois: Charles C. Thomas; Oxford: Blackwell Scientific Publications. 1953. Pp. xv + 157. 32s. 6d.

The study of the psychopathic delinquent comprises the most valuable research in criminological psychiatry as, if for no other reason, it is this individual who has repeatedly precipitated nations into war.

The author believes that there is more to human behaviour than simply the working of a series of chained conditioned reflexes. He holds that a function of the diencephalon is the selection of response. This book, which studies 'a non-psychogenic disorder at a time that interest in psychogenesis is at its

peak', should be read by psychologists as well as psychiatrists, who have been attempting to apply psychogenesis to almost everything. The author's argument is well disciplined.

The specific defect of the psychopathic personality is held to be the inability of the person to develop an adequate concept of time in relation to self. All the secondary symptoms stem from this. It is his disregard for the consequences of action that make the person with a psychopathic personality such a puzzle.

The author adduces much evidence to show that psychological factors seem to have little to do with the aetiology of the true psychopath. The statement by Nielsen and Thompson that 'the most important function of the environment . . . is to inhibit, or augment psychopathic behaviour by the opportunities it affords' stands essentially unchallenged, but environmental psychogenic factors lead to delinquency both in psychopaths and non-psychopaths.

An excellent picture of the characteristics of the psychopathic personality is given in a separate chapter and four main types are described—pathological emotionality, asocial or amoral trends, pathological sexuality, mixed types—with many subdivisions which are not too convincing.

In discussing treatment, a plea is made for separate units or special hospitals for these patients. Psychotherapy benefits only some cases in which the concept of 'partial psychopathy' is applicable. Chemotherapy is frequently useful when there are cortical lesions. The author thinks that perhaps the most hopeful therapy will be found in the field of neurosurgery.

Professor Thompson has made an important contribution to the literature and this book should be studied not only by those who are organically orientated, but more particularly by the psychogeneticists.

J.D.W.P.

Maternal Care and Mental Health. By J. BOWLBY. Geneva: World Health Organization. 1951. Pp. 179. 10s. net.

It is too late to recommend this book to readers for it has recommended itself, and together with the shortened Penguin edition has reached the dimensions of a 'best seller'. The interesting question which arises from this fact is how to account for its popularity and the influence which it has undoubtedly had on public policy.

Dr Bowlby's theme is a simple one. It is that harmful and even dangerous effects can be the result of the separation of small children from their parents. In support of this idea Dr Bowlby collects and documents the work of clinicians and research workers throughout the world.

In this country *Maternal Care and Mental Health* has been widely read by an enlightened public which was shocked at the clearly presented facts about the effects of deprivation. This was the same public which had previously been shocked by the findings of the Curtis Committee, and it seems certain that much of the interest in Dr Bowlby's book is derived from the feelings aroused by the inquiries of that committee.

When *Maternal Care and Mental Health* was published in 1951 enough time had elapsed since the passing of the Children's Act in 1948 for the problems of caring for children deprived of a normal home life to have become evident to all those concerned with the work. The first enthusiasm which had gone into the organization of the Children's Departments within the Local Authorities had worn off as the difficult nature of the task revealed itself. It was just at this moment, when the need for a clearer understanding of the problems of deprived children was keenly felt, that Dr Bowlby's book appeared. It was seized upon and read with avidity by thousands of committee members, administrators and professional workers of all kinds, and there can be no doubt that a new awareness of the emotional needs of children was gained by many people. All those especially concerned with the work of caring for deprived children must be grateful to Dr Bowlby for his timely presentation of knowledge which was urgently needed.

But not only did Dr Bowlby give people the knowledge which they were seeking, he also gave them the feeling that there was something which they could do about the suffering caused by maternal deprivation: they could take more active measures to prevent the separation of children from their parents. The present emphasis on preventive aspects of child care owes much to Dr Bowlby's influence.

Because the fact presented by Dr Bowlby came as a shock to many people, there was a reaction to them, as there always is to shock. Reaction takes the form of either denying the facts, or accepting them to the exclusion of other facts. And reactions of both these kinds are among the snags about a book like Dr Bowlby's, for present policy can be seen to be influenced by people who have reacted in one or other of these ways.

The difficulty is that people who merely react to new facts tend to use them as a basis for argument one way or the other, and in order to gain political ends if necessary. They fail to assimilate the new ideas

and so do not gain the further insight which leads to new development. It is this element of shock in Dr Bowlby's book which could prevent the assimilation of the truths it contains, and could hinder new growth in understanding.

CLARE BRITTON

International Review of Criminal Policy, no. 4, July 1953. New York: United Nations. ST/SOA/Ser. M/4, 19 October 1953. Pp. 110. 15s.

The first two articles deal with sex offenders. G. K. Stürup discusses their treatment in Denmark and the other Scandinavian countries. In particular he discusses the effects of therapeutic castration and concludes that they are exceptionally good. Karl M. Bowman reviews sex legislation and the control of sex offenders in the United States of America. He concludes that it seems better to reform procedural rather than substantive law, premature change in which is always dangerous. In one experimental treatment service psychiatric treatment was supplemented by religious guidance.

Three papers on the treatment of adult offenders in Australia, Venezuela and Yugoslavia are of little or no interest to the psychiatrist or psychologist. There is, however, a comprehensive topical bibliography of current periodical and non-periodical technical literature, concerned with the prevention of crime and the treatment of offenders, comprising 1227 publications.

J.D.W.P.

The Modification of International Attitudes. A New Zealand Study. By J. R. McCREARY. Victoria University College: Publications in Psychology No. 2. 1952. Pp. iii + 153.

This monograph reports a study carried out in New Zealand in 1950 as part of the U.N.E.S.C.O. project 'Tensions Affecting International Understanding'. The aim was to test the effect of knowing more about the Japanese upon attitudes towards them. A Bogardus-type Social Distance Scale was used before and after information had been given to the subjects. One thousand subjects, both adult and child, took part in the first phase of the study, but wastage was high, and only about half of these completed all three phases. The sample can in no way be considered representative of the New Zealand population.

The conditions under which information about aliens was found to affect attitudes include active participation in obtaining such information; the film rather than lecture or booklet mode of presentation; in the case of presentation by speakers, intimate knowledge of the alien group, especially as a member of it, together with equal or superior social status to that of his hearers. The result of presenting 'all sides' of the question was equivocal, and the effect of University classes negligible. Where a national group is the centre of well-established antipathies, specific rather than general information seems to be the more effective.

As the investigators point out, the study is weakened by lack of control groups, and by the short period over which change was measured. No protocols are included to show in detail how the subjects were instructed, and so one can make no guess as to how far the idea of participating in the experiment may have swayed the subjects' opinions. Otherwise, the report is very clear and well organized, and the discussion of remaining problems promises well for future studies from Wellington.

CECILY DE MONCHAUX

The Makah Indians. By ELIZABETH COLSON. Manchester: University Press. 1953. Pp. xvi + 308. 28s.

This excellent book is a study of the degree to which the Makah Indians, a population of some 400 who live in a small reservation on the coast of Washington State, have been assimilated to American culture. Its essential theme is, therefore, not the process by which they have come to lose most of their characteristic customs, but the extent to which they remain and consider themselves a distinct group. They are no longer distinguishable by language, physical type or mode of life from other Americans living among them, and these other Americans must be regarded as forming part of one community with them. Yet they are intensely conscious of their separate identity. The basis of this common consciousness in Dr Colson's view is their identification with the reservation, an area in which they have certain vested interests not shared by white residents. It is maintained by an ideology which represents all whites as actual or potential despoilers of all Indians and as prejudiced against them; the Indian customs which the officials of the United States Government did deliberately suppress during a period of some seventy years are included among the goods of which they have been despoiled, and although no one now remembers the days when they were practised, the knowledge of their having existed is an important part of the

cultural heritage. From her own observations Dr Colson concludes that although there is some social segregation of whites and Indians in the reservation, there is no general prejudice against Indians as a group, such as exists outside and makes it difficult for them to rise above the lowest economic classes. Her study is a valuable contribution to the literature on the position of minority groups; it also suggests depressing parallels in the attitudes of peoples in other parts of the world to whom Western civilization has been brought as an element in alien rule.

L. P. MAIR

Psychological Testing. By ANNE ANASTASI. New York: The Macmillan Co. 1954. Pp. xiv + 682. \$6.75.

The book is divided into four parts. Part I, which occupies 170 pages, deals with the principles of psychological testing. This includes the nature and interpretation of norms, reliability, validity and item analysis. These principles are then applied carefully and clearly to the various tests discussed in the remaining three parts, which are concerned with General Classification Tests, The Differential Testing of Abilities and the Measurement of Personality Characteristics. These sections are given 173, 175 and 136 pages respectively. This perhaps affords some indication of the author's position in relation to Factor Theories. A rough assessment of her general attitude might be given by saying that the evaluations of the many tests would probably yield a pessimistically skewed curve on a three-point scale of Preposterous, Poor and Promising.

The book is written in a style very much more familiar to British readers than is customary in American publications on Psychology. Such readers may, however, be somewhat disturbed at finding no reference to Burt, Thomson or Vernon in the discussion on Factor Analysis and only about three or four references to British tests. Readers in other European countries may be similarly dismayed. If the Monroe Doctrine is to apply to Psychology, Professor Anastasi's book would lose nothing by being re-named *Psychological Testing in America*. In spite of this ruffling of European pride, it is unlikely that serious students of psychological testing will find this book other than skilful in presentation, balanced in judgement and generally much too good to miss.

G. A. FOULDS

The Appraisal of Intelligence. A. W. HEIM. London: Methuen and Co. 1954. Pp. vii + 171. 12s. 6d.

The first page of Miss Heim's book explains that 'appraisal' may be contrasted with *measurement* on the one hand and with *subjective judgement* on the other. In this way the keynote for the whole book is struck firmly at the outset, and the reader is prepared for a sustained attack on the psychometrist, the factor analyst and the group tester. The main thesis of the book is that 'intelligent behaviour may admit of differences in kind as well as in degree', intelligent behaviour consisting of 'grasping the essentials in a given situation and responding appropriately to them'.

Readers of this Journal are well aware of the differences in opinion and attitude between the clinical psychologist and the group tester, between the pure psychologist and the applied psychologist and between the factor analyst and the psychophysical laboratory worker. Each of these contrasts is capable of producing lively controversy: when three of these roles are combined in one person the resulting attack is likely to be pretty strong, and mounted on a wide front. The sins of the psychometrist, the applied psychologist and the factor analyst are described with exhaustive fluency and no quarter is given. The only psychologist to emerge from the fray with credit is Binet—indeed, the author appears at times to lean over backwards in her desire to give full credit to his genius. It is inevitable that in the heady intoxication of combat Miss Heim is occasionally led to the setting up of papier-maché ogres just for the joy of laying them low, and that in a number of places the writing becomes tendentious and perhaps a little querulous.

Yet this is a book which was well worth writing, and one which is strongly recommended for students as well as psychometrists. There are not enough books of this kind available: attacks on intelligence tests and testers are too often written by those who do not know their subject and who are incapable of appreciating the technicalities behind the methods they attack. The chapters dealing with the nature of intelligence and the concepts of mental age and I.Q. will be of considerable value to students, as will those covering reliability and unreliability. These chapters, perhaps, could be expanded a little in any future edition—the consideration of the standard error of score, for example, would be a useful addition. As one might expect from Miss Heim's own research work, the section on speed versus power is of considerable interest. Much of what Miss Heim has to say about validation and its difficulties is excellent, but one feels that the omission of any reference to follow-up studies as a validating technique makes the discussion

less valuable. For the educational psychologist, the 'validity' of an intelligence test is often to be found in its predictive power. Perhaps Miss Heim would find this an inadmissible use of 'validity'—if so, she ought to argue the case.

The final two chapters make more constructive suggestions after the largely destructive criticism of the previous eleven, and here many pertinent and suggestive points are made for future research. It is interesting to note that many of these points—e.g. difficulty of items, speed, the use of creative instead of choice responses—are those which in the educational sphere have recently begun to receive more attention. Miss Heim's suggestion for new methods of appraisal of intelligence will appeal particularly to the clinically-orientated psychologist working with adults. Most of these suggestions need working out more fully, with a particular eye on a number of the criticisms made by Miss Heim herself in earlier chapters. May we hope that these suggestions are an indication of future lines of research by the author?

STEPHEN WISEMAN

Studies of Psychomotor Personality Tests I. By MARTTI TAKALA. Helsinki: Institute of Occupational Health: Ser. B. 81, No. 2. Annales Academiae Scientiarum Fennica. 1953. Pp. 130. 400 marks.

The work carried out at the Institute of Occupational Health, Helsinki, which is reported in this monograph, falls into four parts. The first, concerned with the establishment of factors of motor performance common to different tests, reports results obtained from the factor analysis of variables derived from psychomotor tests given to a group of student subjects. Of the five factors resulting from the analysis, three seem to be of importance. These are identifiable as being concerned with (1) speed, (2) smoothness *v.* disturbance, and (3) suddenness *v.* sluggishness, of psychomotor reaction. The second factor is thought to be related to a general neuroticism factor, whilst the third, it is tentatively suggested, may be analogous to an overactivity-inertia type factor. The second part of the work is concerned with a very thorough analysis of the Mira test, the results of which should be of value to anyone intending to employ this technique. An attempt is made in part three to relate Mira test variables to the three factors outlined in the first part. Results indicated that some wider interpretation of these factors may be necessary. The relation of the psychomotor to other personality tests is treated in part four. This yields some additional information, but on the whole correlation coefficients are not higher than usual in this type of work; a state of affairs which the author rightly says should lead to further development of research methods with attempts to improve the reliability and validity of the tests. The assessment of the work reported is made more difficult than necessary by the use of non-standard scoring methods for tests for which data are available in this country. It is also to be regretted that the work was not carried out within the framework of some general hypotheses, which would have permitted a clearer evaluation of this otherwise careful work.

PETER VENABLES

Growth and Development. By KARL C. GARRISON. London, New York and Toronto: Longmans, Green and Co. 1952. Pp. xii+559. 32s. 6d. net.

This book gives an account of every aspect of child development, physical, intellectual, emotional and social, and provides a good reference book on the subject. Much modern work is described, and the bibliography is most valuable. The form of the book is not perhaps very congenial to British readers, with its list of questions for the students at the end of each chapter. It gives too, a discrete picture of the child's development, and there is not enough emphasis on the interrelatedness of the different functions, nor on the dynamics of their growth and change. Its greatest value would be to the teacher or other child worker in training, as a source book.

A.M.S.

The Child's Conception of Number. By JEAN PIAGET, translated by C. GATTENGO and F. M. HODGSON. London: Routledge and Kegan Paul. 1952. Pp. ix+248. 25s.

This translation, first published in 1952, keeps very close to the original *La Genèse du Nombre chez l'Enfant* and, at the same time, describes the experiments in terms which read quite naturally to an English reader. This is no mean achievement since the environment of the children and, consequently, the small details of the experiments and the idioms in the answers might have seemed somewhat unreal had it been less well interpreted. This seems to have been accomplished with no modification of meaning which I could detect.

The book deals with the development of fundamental bases of arithmetic in children up to the age of about seven years. The stages by which an idea of conservation of quantities is built up are explored in great detail and anyone concerned with the teaching of young children would be well advised to know what is in the first two chapters before attempting to introduce any 'units' into the arithmetic lessons. Four chapters are devoted to probing the growth of an understanding of cardinal and ordinal numbers, in the course of which a most important analysis of the concept of 'seriation' sheds much light on what is involved in a simple ordering of a set of quantities and in establishing a one to one correspondence between two such sets and in the relation between these two processes. The final section is devoted to relations between numbers, but, characteristically, Piaget going more deeply than the simple number combinations, probes into the concepts which must be developed before these combinations can be meaningful. His chapter on the relation between parts and wholes is particularly informative.

This is a thorough and penetrating study of the concepts which are being formed in relation to numbers and quantities at what may be called the 'Infant School' age and, as such, it throws considerable light on the difficulties encountered at this stage when the foundations of future work in arithmetic and mathematics are being laid. The book, however, has a significance far greater than this because it is the first (published in French in 1941) of a series of books devoted to a long and thorough study of the development of arithmetical, spatial and temporal concepts including such books as *La représentation de l'Espace chez l'Enfant* and *La Géométrie Spontanée*. In his earlier works, beginning with *The Language and Thought of a Child* in 1924, Piaget gathered together an enormous quantity of what one may call clinical material, concerning the development of various concepts, beliefs and techniques in children. Piaget was interested in the development of individual children and spurned the 'mean-child' and the standard deviation, making no use of the familiar techniques of statistics to bring one sort of rigour to his researches. The importance of *The Child's Conception of Number* may well lie in the fact that it is the first of a long series of books in which the psychological findings are fitted into a system based on the rigours of mathematical logic. The series culminates in *L'Épistémologie Génétique* (1950) in which he discusses at length the theories of number and spatial relationships, the philosophical basis of his work and the psychological processes involved in an understanding of logical classes and 'groupements', a word which Cattengo and Hodgson translate as 'groupings', but which is used in a special sense and might well have been left untranslated. Considered in relation to the rest of his work, this book marks the beginning of an important stage in Piaget's work and possibly of a fresh advance in psychological theory, particularly in its relation to philosophy.

F. W. LAND

Logic and Psychology. By JEAN PIAGET. Manchester University Press. 1953. Pp. xx + 48. 6s. net.

In this little book, based on three lectures given in Manchester in 1952, Piaget indicates, in a thumbnail sketch, the very important and significant light which, in his view, modern logic can throw on genetic psychology. His position briefly is that modern operational logic can supply just the required explanatory theory for all the observed facts (as established and recorded by him and his co-workers over the last thirty years) of the stages, the diverse aspects and the final outcome of the great, long-drawn-out process of intellectual development in the child. In particular the 'algebra of logic'—translated into a suitable intermediate 'psycho-logic' or 'logico-psychology'—can make intelligible to us why various *seemingly unrelated* types of achievement present themselves *at the same time*, at the age of about twelve: in the light of this algebra of logic they now all fall into place and we can see them as the naturally correlated members of a single whole structure or system.

The master-key for Piaget lies of course in the notion of operations. This is for him common both to psychology and to logic and the crucial link between them. After a brief account of the 'History and Status of the Problem', he outlines in his first main psychological chapter, how up to the age of about seven years the ground is prepared; how from 7 to 11 years operations of 'combining, dissociating, ordering and the setting up of correspondences' arise and develop, but are only applied concretely to objects themselves; but how from 11–12 to 14–15 years these operations become internalized, are applied to propositions instead of physical objects and above all achieve the status of 'structured wholes' within which the all-important property or law of *reversibility* holds. At the same time, however, there appear the above-mentioned new groups of operations of 'operational schemata' without any obvious relation to the 'logic of propositions': combinations and permutations, proportions, conceptions of mechanical equilibrium, probabilities, etc. These present us with an intriguing psychological problem, but here Piaget finds the illuminating solution in the 'algebra of logic' which enables us to understand the nature

of the system of logical equilibrium towards which our whole intellectual evolution must tend. Accordingly he goes on, in his next chapter, to give a summary (and necessarily technical) sketch of the algebra of logic, and in the final chapter shows how this serves to explain first, the 'outline structures' of the 7-8 year level, and secondly, the full structure of *reversible transformations* of every kind governed by the laws of equilibrium and representing the attainment of mature adult logic.

At various points in the exposition controversial questions arise, but within the compass of three lectures Piaget could hardly aim beyond a simple introductory presentation of his point of view. Its solid foundation must depend, on the one hand, upon the vast body of his practical work, and on the other, upon his comprehensive theoretical treatises on logic and on genetic epistemology. How conclusively these establish every part of his position falls outside the scope of this brief notice. But it may be hoped that the present thought-provoking introduction will, for many students, serve as at least a gateway into a great world of cumulative psychological exploration and understanding, the significance of which has not yet, in the writer's view, been properly appreciated. Piaget's thirty years of systematic investigation and mapping of the developmental history of the child's mind, in every one of its intellectual aspects, will, I believe, be acknowledged in future days, if not at the present time, as far and away the most important body of contributions to cognitive psychology within our generation. NATHAN ISAACS

Statistical Methods in Experimentation. By O. L. LACEY. New York and London: Macmillan and Co. 1953. Pp. xiv + 249. \$4.50.

A Statistical Primer. By F. N. DAVID. London: Charles Griffin and Co. 1953. Pp. xi + 226. 22s. net.

Both these books are text-books for subsidiary courses in statistics. Prof. Lacey's (University of Alabama) is intended for use in a department of psychology; Dr David's (University College, London) is provided for a more miscellaneous audience of 'post-graduates and undergraduates drawn from nearly all science departments and possessing little in common except an ignorance of mathematics'. Actually both assume their students have reached about the same level, algebra up to General Schools Certificate standard approximately, though Prof. Lacey evidently hopes a few of his students may have been introduced to integral calculus.

Both describe properties of the normal univariate frequency distribution and methods of testing the significance of differences between samples. Dr David goes on to consider binomial and Poisson distributions in some detail and concludes with uses of the chi-squared test. Prof. Lacey deals much more briefly with enumeration statistics and devotes his later chapters to properties of bivariate frequency distributions—correlations, regression coefficients, etc.

Perhaps it is impossible to convey complete conviction while rigorously avoiding mathematical proof. Dr David adopts an impressively authoritative manner, Prof. Lacey is more sweetly reasonable and persuasive, but neither succeeds completely. For instance Dr David, on the comparative merits of means, medians and modes, tolerates little argument: 'The first measure we shall discuss and the one we shall afterwards always use is the mean... The real reasons why statisticians do not favour (the median) are that it is mathematically intractable and tends to be more unreliable than the mean... The same objections can be made against the mode.' In contrast, Prof. Lacey's presentation of the formula for product moment correlation (abbreviated here) gently leads the reader along: 'A measure of the degree to which values above and below the average are associated is immediately available by the simple device of multiplying together paired deviations... The simple sum of the cross products will be an index of the degree of correlation... It is plausible then that the first step is the calculation of Σxy . Now it is clear that this sum is not immediately the correlation coefficient itself. It must be compared with some other figure also dependent upon the data. It may occur to you that it could be compared with either Σx^2 or Σy^2 ... The value which would seem reasonable is the average of the two... it is, however, the geometric rather than the arithmetic average... So our correlation coefficient is finally defined', etc. In both contexts an attempt to define the criteria of the efficient statistic might have been made. Without it, statisticians' preferences must appear rather arbitrary to the student.

A great variety of problems have to be met in preparing lectures for such subsidiary courses. Though there is some agreement about the subject-matter to be included, no plan is likely to prove equally satisfactory in all localities. To hold his students' interest the lecturer will probably prefer to borrow his examples from experiments conducted concurrently in his own department, and adapt his computing methods to the apparatus available there, integrating his course with the rest of the work in every way he can. So though other lecturers may find it well worth while to study how these authors present their

subject-matter, I cannot estimate how widely either book will be adopted as a text-book. Students who wish to brush up statistics for an examination may perhaps find Dr David's severely practical text more suitable for their needs; those who are not yet sure how far statistics can help them in their other studies may find Prof. Lacey's the more encouraging.

PATRICK SLATER

Psychical Research Today. By D. J. WEST. London: Gerald Duckworth and Co., Ltd.
Pp. 144. 1954. 12s. 6d.

This little book is an attempt to describe briefly the present state of psychical research and the main problems which are now engaging the attention of students of the subject. Dr West is commendably cautious in his approach and the beginner will find in his work a useful text-book from which to start a further inquiry. In nine concise chapters, supplemented with useful and well chosen documentation, the author sketches the principal phenomena and subjects the evidence to critical analysis.

In his treatment of spontaneous psychic impressions Dr West shows himself keenly alive to the weakness of much of the evidence, although it is somewhat surprising that he omits to mention the early criticism of Gurney and his associates in their records of alleged veridical phantasms. In his account of the phenomena of the seance room the author exhibits a scepticism much greater than that he displays in his treatment of extra-sensory perception. It is difficult here to contradict anything that he writes: the melancholy story of fraud by mediums and suppression of the facts by psychical researchers is only too well known. Perhaps a wider acquaintance with the physical phenomena might have led him to modify some of his conclusions, or at least to have omitted one or two pages and inserted in their place a discussion of the best evidence.

It is in his treatment of extra-sensory perception that the author exhibits his keenest appreciation of modern tendencies. He is of the opinion that 'almost everyone is agreed' that E.S.P. has been shown to exist and in dealing with the evidence for it he shows himself well acquainted with the literature in English and the experimental procedure. Generally speaking, his treatment is restrained, although it is curious, if not a little disturbing to find that among the tests he thought worth recording was the American experiment conducted by Professor Riess. After describing the test and the amazing accuracy of the subject's guesses, he concludes by saying that 'it is perhaps unwise to pin too much faith upon it'. It is indeed.

The book is well printed and indexed but is somewhat marred by the illustrations which it is difficult to suppose could have been chosen by Dr West and should not have been sanctioned by him. Nevertheless, the book is to be commended to all interested in these matters and students should note one of the author's final conclusions that 'the whole subject is one big question mark'. Few, certainly, could quarrel with that.

E. J. DINGWALL

The Doors of Perception. By ALDOUS HUXLEY. London: Chatto and Windus. 1954.
Pp. 64. 6s.

Most psychologists have probably at some time or another become interested in mescaline, either by experiment and experience, or at least by some study of the large literature which has accumulated concerning results following its administration. Also most psychologists will be familiar with the claims that have been made that substances chemically closely related to mescaline can produce effects similar to, or identical with, some of the leading symptoms of schizophrenia. It is important to remember that Mr Huxley was apparently well acquainted with much of this earlier work, and that in particular he was already deeply interested in the adenochrome experiments before he tried to discover something about the effects of mescaline upon himself. What he did was to take four-tenths of a gramme of mescaline dissolved in half a glass of water, and then to record observed results in detail, using verbatim automatic recording whenever possible.

The essay, deeply interesting to read, is literary in manner, with here and there bits in the scientific style which do not altogether fit in well with the rest. It is not at all easy to tell how much of what Mr Huxley has written is due to reflexion upon his actual first-hand experience, or to the literary and stylistic demands themselves, and how much of it is really literal report. He claims that an hour and a half after he took mescaline a lot of quite ordinary things—flowers in a vase, books on his study shelves, a plain typing table, folds in his trousers, were seen as possessing some vast and tremendous, though not readily definable, significance. To Mr Huxley, then, and apparently afterwards as well, this seemed to be 'better' perception, revealing something which is known to be real in some intimate and unquestioned sense, for the description of which the perceiver must draw upon the language of mysticism.

Later, Mr Huxley moved about, in a garden and in the larger outside world. Neither space nor time seemed very important, apparently because 'there is such a lot of them'. The distinction between the world outside and the private world still remained, but the sort of world Mr Huxley got when he shut his eyes—'brightly coloured, constantly changing structures that seemed to be made of plastic or enamelled tin'—appeared to him to be cheap and trivial.

This is one of the places at which the essay is not very clear. A little later the schizophrenia theme comes in. And then it seems as if an outward directed perception—in this case of a garden chair, for example—may yield something that is ineffable, supremely important, and neither 'inward' nor 'outward'; for there simply is nothing else. Initially, it seems to be held, this is the heavenly phase of schizophrenia, but it may go on to be compulsive, obsessive and may then turn into the 'hellish' phase. But Mr Huxley is satisfied that mescaline does not take anybody that far unless he has had a recent attack of jaundice.

The final part of the essay is concerned to discuss the ways in which mescaline and similar drugs might be made a valuable adjunct of religious ceremonial. It might 'shake people out of the ruts of ordinary perception' and so make more accessible a world of values far removed from the conventions of common sense. It seems to be implied that, whether by the help of mescaline or some other drug, whether by the 'sunset touch', or by some special sensitivity to sound or shape or colour unmediated by anything else, everybody ought to get into this world from time to time and nobody ought to stay in it very long.

In a technical, scientific way there is very little psychology in this book, and 'perception' in particular is being used in the special sense of some function which is, so to speak, capable of absorbing, and being at one with, whatever, at other times, it would be said to be directed upon. But psychologists should read this essay, not merely because most of it is beautiful writing, but also because it raises again those old issues of how, if it can be done, the values of those formulations which pass as the everyday currency of meaning from person to person, may be reconciled with the other values of becoming-aware-of meanings that are not, and perhaps cannot be, formulated at all. The issues are raised, but they are not revolved.

F.C.B.

A Survey of Public Opinion in Relation to the University. By MARJORIE N. DONALD.

Publications in Psychology No. 4. Department of Psychology, Victoria University College, Wellington, New Zealand. 1953. Pp. 90 + lviii, Appendices. No price given.

The survey in this booklet was the practical outcome of an undergraduate course on the design and administration of public opinion surveys. The students participated at all stages, from the construction of the interview schedule to the analysis of results.

The aim of the research was to uncover the views held by a stratified sample of 400 people in the capital city of Wellington, on the subject of the University and its function within the community. Among the topics investigated were the following: Who should enter the University? What value is placed upon a University education? How does the behaviour of University students compare with that of other young people of the same age?

Detailed breakdowns of opinion in all areas are clearly presented and indicate that the local college is held in high esteem by the people of Wellington. Among the many interesting findings were two strongly held views, 'that a university training equips a student to become a better citizen than he otherwise would be' and 'that the value of a girl's university training remains with her after she marries.'

ALAN RICHARDSON

Authority and Freedom. By R. H. THOULESS. London: Hodder and Stoughton. 1954.

Pp. 124. 8s. 6d.

This, addressed to theologians, is intended to be a contribution to reversing the 'decline in religious conviction'. It gives brisk support to the Creeds and to the idea of dogma but recommends the (Anglican) Church to treat most of its doctrines as 'orthodox but non-essential beliefs'. Religious intolerance is deplored in a chapter which also gives some very fine arguments for it.

The first chapter says that there has been a decline in church membership, excepting that of the Roman Church; that figures of such membership are a good measure of religious conviction; that there are indications that the decline has been somewhat reversed since 1935. It suggests that the social consequences of this decline may be undesirable; says that it is not known that delinquency is increasing but that there is some reason (not given) for thinking it may be and that the decline is a cause of the increase; it says

that religion should be taught not as a means of good citizenship but because it is true. There is argument here of the sort that was roundly condemned by Dr Thouless in 1939 as 'fantastic anecdotalism'. He does not wonder *why* there has been no decline in Roman Catholicism nor what is the incidence of maladjustment among Catholics.

The last chapter attempts to resurrect the traditional arguments for the existence of God as *inductive* arguments having a probability, a howler of a 'category-mistake'.

Liberal-minded churchmen will like this book: other Christians may derive comfort but no help in recruitment, for it is not that unbelievers mistake the meaning of 'will' in 'Whosoever will be saved...' but that they have no idea what 'be saved' means, and this is *not* because they have lost the ability to understand metaphor (cf. chapter 5). Dr Thouless is described on the dust-cover as Reader in Educational Theology.

JOHN SYLVESTER

Jung's Psychology and its Social Meaning. By IRA PROGOFF. London: Routledge and Kegan Paul. 1953. Pp. xx+299. 18s.

The aim of this book is stated as the interpretation of Jung's ideas in such a way that they can be 'analysed, mulled over, criticized, appreciated, and most important, *worked with* as hypotheses in related fields of psychological and social study.' The exposition has been handled ably, as anyone who has struggled with the works of Jung would wish to testify. On the critical side some disappointment may be felt. For instance, the author shows that Jung has been more concerned with the social foundations of man's life than is commonly supposed, but he does not consider the influence of nineteenth-century thought in producing a confusion between social and biological inheritance in Jung's distinctive concept of the Collective Unconscious; on the contrary, the language used suggests an acceptance of the Jungian position. In general, the inconsistencies of Jung's writings are acknowledged more openly than is usual with his sympathizers and supporters, but their customary standpoint is maintained, i.e. that the value of his work lies mainly in 'an attitude toward the reality of the psychic life and the validity of the spiritual experiences of man in history'; and that the material which he studies needs to be experienced not only by the intellect but 'more comprehensively, by the personality as a whole'. In view of the aim as declared, more might have been done to determine, point by point, what the set of values really is which Jung is endeavouring to convey, and to explain in detail how his concepts are to be used as hypotheses in the study of specific problems. One of the most interesting features of the present work is a chapter on 'The Historical Situation', based partly on conversations with Jung himself, in which the development of his ideas and his relationship with Freud are treated more fully than in other published sources.

A. M. DRY

Steps in Psychotherapy. By JOHN DOLLARD, FRANK AULD, Jr., and ALICE MARSDEN WHITE. New York: The Macmillan Company. 1953. Pp. ix+222. \$3.50.

This book is disappointing. It achieves its stated aims—the presentation of seventeen hours of therapy on Mrs B. whose fundamental 'sex-fear conflict' manifests itself as a 'spouse-phobia'. Transcripts of the sessions are given, with a commentary from an arm-chair supervisor. In the Brief Psychotherapy described, no fundamental change in psychotherapeutic technique is presented but 'merely a limited attack on the total psychotherapeutic problem'...(which)... 'must necessarily deal with everyday things'. Thus, to its brevity can be added the characteristic, 'superficiality', and we have the conventional supportive therapy practised in most of our busy psychiatric outpatient departments.

The disappointment comes from the fact that while the authors state that their therapeutic techniques are based on the Dollard and Miller blend of reinforcement learning theory and psychoanalysis, the comments and records of the sessions show only a little Freud, less Hull, and no Dollard and Miller.

The appearance in 1950 of *Personality and Psychotherapy* by Dollard and Miller was hailed as a 'marriage of psychoanalysis and learning theory'. The technique revealed in this book does little to reassure us that the union is potentially fruitful, or even happy.

The final two chapters discuss the relevance of the psychological tests given to Mrs B. The reports of the clinical psychologists are equivocal. The authors feel that this is because of the lack of validity studies of such tests 'utilizing criteria pertinent to personality diagnosis and therapy'. This is all too true. But the handling of Mrs B.'s case does not demonstrate how psycho-analysis strengthened by the rigours of learning theory can provide any such adequate criterion.

R. M. MOWBRAY

Psycho-analysis and Child Psychiatry. By EDWARD GLOVER. London: Imago Publishing Co. 1953. Pp. 42. 6s. net.

This is an expansion of an article previously published in *Samiksa*, the journal of the Indian Psycho-analytical Society. It is expanded, but not very much. The mental apparatus in general, constitutional factors, clinical criteria, classification, differential diagnosis and treatment form the subject-matter. So much in so short a space tends inevitably perhaps to generalizations without their supporting evidence and these are certainly not lacking. They are perhaps particularly unfortunate in a book that starts off with a very firm censure of the child analysts—Mrs Klein and her followers especially—on just these grounds of failure to produce evidence for their views. A great body of work by child analysts, which many feel to be a major contribution of psycho-analysis to child psychiatry, is in fact set aside.

To those already familiar with the author's other writings, in which his views are more fully supported, this book will be of interest. To others the succession of rather sweeping statements is likely to carry very much less conviction.

Fundamental Concepts in Clinical Psychology. By G. WILSON SHAFFER and RICHARD S. LAZARUS. New York. McGraw-Hill Book, Co. 1952. Pp. 540. \$6.00.

This book sets out to cover the basic principles, methodology and general techniques that are the concern of the clinical psychologist, with a view to providing an introduction and supplement to more specialized studies. The field of the clinical psychologists, as here described, is clearly very much broader than would be normal in this country and indeed it is sometimes difficult to see what, if any, are its limits. A wide range of methods of psychological diagnosis and of therapy is discussed. To those entering on the study of mental health this book may prove useful, provided it is realized that the techniques in question are in fact the tools of a number of specialists rather than those of any one profession. There is a serviceable bibliography of the American literature with only scanty reference to that of other countries.

Psychiatric Aide Education. By BERNARD HALL *et al.* Menninger Clinic Monograph Series, No. 9. New York: Grune and Stratton. 1952. Pp. xviii + 168. No price given.

It appears that the 750,000 hospitalized mental patients in the U.S.A. are nursed by some 90,000 personnel of which only 10,000 are trained nurses. The rest are 'attendants', or 'psychiatric aides' as they are now called. Training schemes for psychiatric aides have existed in many American mental hospitals since about 1900, but these have had the effect of turning 'aides' into 'nurses' who, in turn, have tended to leave for more lucrative work elsewhere. In any case, only a small proportion of aides took the courses provided.

In order to help to remedy this state of affairs, the Menninger Aide School was opened in October 1949, which trained a total of sixty psychiatric aides in four intensive courses until it closed in 1952. The purpose of this School was to investigate the material that should be taught and the most suitable methods of teaching it, and also to determine the best selection and recruitment procedures. It was also hoped to graduate a small number of competent aides who would emerge as leaders for the psychiatric aide group. The book is a report of this investigation.

The results reported have only slight relevance to conditions in this country. In British mental hospitals the male 'attendant' is now unknown. All male nursing staff are either student or graduate mental nurses, and senior nursing staff usually have a general training as well. Female 'nursing assistants' are employed but, they always work under the direct supervision of fully trained senior nurses. Some mental hospitals have a training course for such nursing assistants, followed by an examination and a Hospital Certificate. The syllabus outlined in this book seems too advanced for nursing assistants yet not adequate for fully trained mental nurses.

RALPH HETHERINGTON

Diagnostic Casework in the Thomastic Pattern. By MARY J. McCORMICK. New York: Columbia University Press. London: Geoffrey Cumberlege. 1954. Pp. xvi + 239. 28s. net.

It may be a matter of surprise to many psychologists that a theologian can contribute to our understanding and practice of case-work. But Miss McCormick proves otherwise. In this country we tend not to consider diagnostic case-work as a subject separate from either social case work or psychotherapy, but

it is clear, if we make the distinction, that the social worker meets many problems which are neither strictly psychiatric nor administrative, but educational and moral. It is in these spheres that the profound human understanding of Thomist philosophy makes a great contribution, whether or not one follows it to its ultimate Christian conclusions. It is a philosophy which emphasizes at once the failings and the grandeur of human personality, and stresses the duty we have to each other to release the nobility from the pains of the failings, for 'the sum total of the Christian religion consists in mercy, as regards external works'. Miss McCormick has breadth and wisdom in her approach, while her counsel of prudence could well be more widely emulated.

Altogether, this is an excellent, if not a great book; and quite apart from its metaphysics, which will be stimulating to some and meaningless to others, it should prove of real value to the practical social worker.

ADAM CURLE

Psychology, the Nurse and the Patient. By DORIS M. ODLUM. London: 'Nursing Mirror'. 1952. Pp. 114. 7s. 6d. net. 2nd edition, 1954. Pp. 168. 12s. 6d. net.

Dr Odum's book is an attempt to fill the need for a comprehensive and readable introduction to psychology for nurses in training, and in this second edition, much new material has been included. Selection of topics and the amount of space and importance given to them are always difficult in a book of this kind, and the difficulty is particularly noticeable here. One could wish that more space had been given to purely psychological matters and less to details of nursing care, and that the new material had been incorporated better into the body of the book, so that repetitions and bad references were avoided. The most valuable part of the book is that on the nurse's relationships in her work, and for that reason, the book should prove useful for the nurse in training, but not as her only reading in psychology.

A.M.S.

Issues in the Study of Talent. By D. W. BRAY. Columbia University Press; London: Geoffrey Cumberledge. 1954. Pp. xi+65. 16s.

It is difficult to understand why this small book was produced—particularly at such a price. It is intended to contribute towards the 'conservation of human resources' research project of the Columbia University Graduate School of Business, and sets out to 'develop a theoretical framework that will provide guide posts for future work'. It tries to do this by giving a brief and superficial review of the nature of intelligence, the work done on heredity and environment, the evidence of factor analysis and the relevance of motivation in work performance; then a 'research plan' is produced which depends very little upon the previous outline. The emphasis in the book is on the importance of environment: 'The average innate capacity of children in one socio-economic-cultural group does not differ substantially from the capacity of children in another group. Therefore, the wide differences in I.Q. among children from the different groups must be due primarily to differences in cultural opportunity'. A review of factor analysis leads to the conclusion that *g* reflects the cultural heritage of the society, and that Vernon's *v:ed* and *k:m* group factors arise from children using their 'general learning potential' either inside or outside school.

It is proposed 'to study individuals who have achieved prominence through their superior performance in music, physical science, or business administration'. The possibility of using professional baseball players is discussed and then—reluctantly?—abandoned. It is hoped to discover factors connected with the recognition, development and utilization of potential.

If the fashion of writing books describing what researches are *going* to be done becomes common, the task of reviewers is likely to become less and less attractive.

STEPHEN WISEMAN

OTHER PUBLICATIONS RECEIVED

- EMIL A. GUTHEIL. *The Handbook of Dream Analysis*. New York: Liveright Publishing Corporation; London: George Allen and Unwin. 1952. Pp. 710. 48s. net.
- Interrelations of Cultures*. Paris: U.N.E.S.C.O. 1953. Pp. 387. 14s. 6d. net.
- KURT REIDEMEISTER. *Die Unsachlichkeit der Existenzphilosophie*. Berlin: Springer-Verlag. 1954. Pp. iv+40. DM 4.80.
- KURT REIDEMEISTER. *Geist und Wirklichkeit. Kritische Essays*. Berlin: Springer-Verlag. 1953. Pp. iv+92. DM. 8.60.
- GÜNTER CLAUSER. *Die Kopfuhr; Das automatische Erwachen*. Stuttgart: Ferdinand Enke Verlag. 1954. Pp. viii+108. DM 11.80.
- PETER R. HOFSTÄTTER. *Einführung in die Sozialpsychologie*. Stuttgart: Humboldt-Verlag. 1954. Pp. 536. DM. 12.50.
- Index Bibliographicus*. Vol. II. compiled by THEODORE BESTERMAN. Paris: U.N.E.S.C.O. 1952. (3rd edition.) Pp. xi+72. 9s. 6d. net. (H.M.S.O.)
- ERNEST, L. REMITS. *The Feeling of Superiority and Anxiety—Superior*. Ottawa: Runge Press Ltd. 1953. Pp. x+67.
- PIERRE PICHOT. *Les Tests Mentaux*. Paris: Presses Universitaires de France. 1954. Pp. 128.
- Aids to Educational Research*. (New Edition.) Publications of the Scottish Council for Research in Education xxxi. London: University of London Press. 1954. Pp. viii+42. 2s. 6d. net.
- The Teaching of the Social Sciences in the United States*. Paris: U.N.E.S.C.O. 1954. Pp. 150. 6s. net.
- WILLIAM V. SILVERBERG, M.D. *Childhood Experience and Personal Destiny*. New York: Springer Publishing Company, Inc. 1952. Pp. xiv+289. \$4.50.
- JOSEPH NUTTIN. *Psychoanalysis and Personality*. London: Sheed and Ward. 1954. Pp. xiv+310. 16s. net. (This is an English translation of the book reviewed in the *British Journal of Psychology*, vol. XLIII, pt. 3, August 1952.)
- A General Selection from the Works of Sigmund Freud*. Edited by JOHN RICKMAN (Psycho-Analytical Epitomes No. 1.) London: The Hogarth Press. 1953. Pp. x+332. 10s. 6d. net.
- SIGMUND FREUD. *Civilisation, War, and Death*. Edited by JOHN RICKMAN. (Psycho-Analytical Epitomes No. 4.) London: The Hogarth Press. 1953. Pp. xviii+118. 8s. 6d. net.
- Cultural Patterns and Technical Change*. Edited by M. MEAD. Paris: U.N.E.S.C.O. 1953. Pp. ii+350. 10s. 6d. net.